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STONE DISKS AS TREATY "SUNS"

WALTER ABELL

ONE of the minor mysteries of American archaeology concerns the use made by the Indians of certain stone disks found in the Mississippi Valley and the Southeast. According to Charles C. Jones, Jr., the first of these disks was "ploughed up in 1859, on the lower terrace of the large temple-mound on the Etowah River . . . near Cartersville,"[1] Georgia. Other discoveries followed, and several scores of the disks are now in archaeological collections.

Many conjectures have been made as to their function. In 1873, Jones suggested that they were ceremonial plates on which food was offered to the sun god. "We incline to the opinion," he wrote, "that these stone plates were designed for sacred uses, and that in them was exposed the food offered to the Dii Minores of those peoples who, antedating the modern Indians—dwellers here at the dawn of the historic period—erected the large temple-mound in honor of that great God who mingled not with men, and before whose flaming minister—the sun—they prostrated themselves in blind yet profound adoration."[2] G. P. Thruston, while accepting Jones' view of the more elaborate disks, thought that "some of the plainer forms . . . were probably used for domestic or culinary purposes."[3]

Most of the disks have notched or scalloped borders (Fig. 1 and Pl. I). This fact suggested the possibility that the notches might have some relation to divisions of time. In 1875, W. M. Anderson pointed out a certain similarity between one of the disks and the Toltec calendar.[4] W. H. Holmes also suggested that these objects might be calendar disks. "The student," he said, "will hardly fail to notice the resemblance of these disks to the calendar stones or time symbols of Mexico and other southern nations of antiquity."[5] In 1904, H. R. Stoddard said of a particular disk, the border of which is formed of thirty-six notches, "That the engraved discoidal was ever used as a sun dial or measure of time is problematical; that it could have been used as such, accurately, is self-evident."[6] He accordingly concluded that the disk in question was presumably designed for such a purpose.

Unfortunately, all theories relating the disks to the measuring of time break down when an attempt is made to apply them to the class of disks as a whole. No single disk can be identified completely with any known calendar, and no two agree in their system of notching.

In many cases the number of notches on a disk is an irregular one involving no connection with recognizable intervals of time. The notches vary in number on different disks from seven at one extreme to seventy-two at the other, and are found in such seemingly haphazard numbers as fifteen and twenty-three; in one instance, fifty-nine small notches are irregularly subdivided by seven large ones. Such varied and, it would seem, numerically uncalculated notching suggests that the notches have no intentional relation to time and must have been made in some other connection.

These stubborn facts soon discouraged the proponents of theories related to time measurement. Even in their initial statements of the calendar hypothesis, both Anderson and Holmes were forced to admit the difficulty of pressing it to a satisfying conclusion. "I have fancied a resemblance," said Anderson with regard to his comparison of the disk with the Toltec calendar, "but I cannot establish a complete agreement between the two tablets."[7] He then enumerated the difficulties involved in trying to force the intractable notches into relation with a time system. Holmes recognized the same difficulties, remarking with regard to his calendar suggestion, "There is, however, no absolute identity with southern examples."[8] Eventually most

[1] Jones, 1873, p. 373. [2] *Ibid.*, p. 376.
[3] Thruston, 1897, p. 275.
[4] Quoted in Page, 1875, p. 378. [5] Holmes, 1883, p. 279.
[6] Stoddard, 1904, p. 156.
[7] Page, 1875, p. 378. [8] Holmes, 1883, p. 279.

students of the subject, including Holmes, abandoned any attempt to connect the disks with time measurement.

The theory which has found most general acceptance was put forward by Clarence B. Moore in 1905. "Stone discs and slabs were found by us on many occasions at Moundville . . . and in each case the disc or the slab was more or less thickly smeared with paint, sometimes cream-colored, sometimes red . . . . It seems conclusive to us that the paint on the discs and slabs is purely of aboriginal origin.

"The universal presence of paint upon these discs and slabs seems to offer a clue to the purpose for which they were used, and, until a better suggestion is offered, we shall consider them palettes for mixing paint."[9]

A year later, that is to say in 1906, Holmes published a paper devoted to the disks in which he accepted Moore's theory in preference to his own previous suggestion. "In a recent work," Holmes wrote, "Mr. Clarence B. Moore illustrates a number of discoidal and rectangular stone plates obtained from mounds in Alabama and elsewhere in the South, which he is able to identify as mortar plates, or palettes, intended for the grinding of pigments. It thus happens that another of the several groups of archaeological objects heretofore placed in the problematic class is safely assigned to a definite use, although the exact manner and significance of the use remains in a measure undetermined. The rectangular plates bear a more or less marked resemblance to the flattish rectangular tablets employed by Pueblo shamans in grinding pigments for sacred purposes; and several of the mound specimens, both rectangular and circular, as demonstrated by Mr. Moore, bear unmistakable evidence of use in preparing colors, a sufficient amount of the pigment remaining on the surface to permit technical analysis. The colors are for the greater part red and white, the former being hematite and the latter carbonate of lead."[10]

Subsequent writers for the most part have followed Moore and Holmes in this conclusion, though still regarding it in the nature of an hypothesis rather than an established fact. Thus, Douglas and d'Harnoncourt, in their *Indian Art in the United States*, remark that the disks "are believed to be palettes on which paint was ground because traces of red and white paint have actually been found on some of them."[11] Webb and DeJarnette, in *An Archeological Survey of the Pickwick Basin*, repeat the same view of the function of the disks. "Some of them," they state, "are concave on one face as if used as palettes for grinding paint. Many of them have been found with lead or iron oxides marked on them."[12]

On the other hand, dissenting opinions, though in the minority, have not been entirely lacking. Moorehead observes that "the discs owned by Mr. Stoddard have not smooth centres as have Mr. Moore's discs, and are apparently for other purposes than the mixing of paint."[13] The most recent of all mentions of the discs to come to my attention reverts to a noncommittal attitude regarding their possible function. I refer to Waring and Holder's statement: "They have been variously referred to as paint palettes, 'sun circles,' and even calendar stones. At present it is futile to suggest a function for these objects."[14]

In reading *The History of the Five Indian Nations* by Cadwallader Colden, I chanced upon two passages which suggest that the disks may have been made for a purpose not previously considered, namely, for use as pledges of faith in the ratification of certain articles in peace treaties.

Colden's book, first published in New York in 1727, is an account of the relations between certain Indian tribes, and between those tribes and the white colonists, during the seventeenth and eighteenth centuries. A considerable portion of it is devoted to verbatim reports of Indian council meetings written during the progress of the meetings by a colonial secretary and preserved in New York's Register of Indian Affairs. These reports repeatedly record the Indian custom of laying down a symbolical gift—usually a wampum belt, but sometimes a pipe, a beaver skin, or other object—as a pledge of faith in making any serious proposition or ratifying any agreement. Among the Indians, the presentation of such a gift had the same force as does the affixing of official seals or signatures in our own society. With the gift, the agreement was consummated; without it, no final obligation was undertaken.

Of the hundreds of signatory gifts mentioned

[9] Moore, 1905, pp. 145–147. [10] Holmes, 1906, p. 101.

[11] Douglas and d'Harnoncourt, 1941, p. 87.
[12] Webb and DeJarnette, 1942, p. 287.
[13] Moorehead, 1910, Vol. 1, p. 453.
[14] Waring and Holder, 1945, p. 13.

in the documents quoted by Colden, two are stone disks. Reference to the first of these disks occurs in an account of a general Council of the Five Nations which opened at Onondaga on January 22, 1670. At one session of this council, says Colden, "Cannehoot, a Seneka Sachem stood up, and gave the general Council a particular Account of a Treaty made last Summer, between the Seneka and the Wagunha Messengers, (one of the Utawawa Nations) who had concluded a Peace for themselves, and seven other Nations to which the other four Nations were desired to agree, and their Brethren of New York to be included in it. He said the Proposals made in several Propositions were as follow."[15]

After four articles, confirmed respectively by the delivery of two prisoners and the giving of three belts, a fifth proposition is recorded in the following words.

"5. 'Let the Sun, as long as he shall endure, always shine upon us in Friendship.' Here he gave a red Marble Sun as large as a plate."[16]

A similar incident took place twenty-four years later, in a council held in Albany in August, 1694. There, on the seventeenth of the month, an Onondaga Sachem, Sadakanahtie, "after he had sung a long song," presented a peace offer sent by the Dionondadie tribe. After several clauses, confirmed respectively by "a few Strings of Wampum" and four belts, the statement reads:

"Brethren, (we include all the Nations from the Seneka's country to New York in this Name) hearken to us. We rend the Clouds asunder, and drive away all Darkness from the Heavens, that the Sun of Peace may shine with Brightness over us all; giving a Sun of a round red polished Stone."[17]

In these two passages we have verbatim seventeenth-century accounts of the use of stone disks as signatory objects. In both cases the disk replaces the more customary form of sanction, such as the wampum belt, for a representational reason. The agreement to be confirmed is a promise of peaceful relations between nations. In the wording of this agreement, the sun is invoked as a symbol of peace, in contrast to the "clouds" of war. An object recalling or suggesting the sun would therefore be particularly appropriate as a confirmation of these clauses, and such an object is provided in the form of a stone disk of a warm color. In both passages the disks are designated "suns."

The question at once arises as to whether the stone disks now preserved in archeological collections may not be objects of the same class as the "suns" used on these two occasions. In an effort to answer this question, let us reconsider the disks in the light of the information contained in, and the possibilities suggested by, Colden's account.

In Colden's first reference the "sun" is said to have been "as large as a plate." This corresponds well with the size of the existing disks. Of the eighteen examples listed by Webb and DeJarnette, ten are from 7+ to 8+ inches in diameter. This is about as close as they could come to being "as large as a plate." Only two are smaller than this, the smallest being 4.5 inches in diameter. Six are larger, the largest being 12.5 inches in diameter. Even these extremes do not exceed the limits of what we commonly designate as "plate" sizes.

The materials of the two disks mentioned by Colden are described as "red Marble" and "red polished Stone." The secretary who made the original records at the council meetings had, of course, only an incidental interest in the nature of the objects used as pledges. One might expect his descriptions to be based on offhand observation rather than scientific examination. "Marble" would probably mean to him any material recalling that substance. That this is so is suggested by two other passages in which Indian pipes are described as made of "marble." Thus, in the address of Cannehoot, from which we quoted the reference to the "red Marble sun as large as a Plate," the succeeding clause reads as follows: "6. Let the Rain of Heaven wash away all Hatred, that we may again smoke together in Peace, giving a large Pipe of Marble."[18] It seems evident that the term "marble" is loosely used to indicate what the second and more literal account describes as "red polished Stone." The words of another contemporary observer confirm this interpretation, for Marquette describes a calumet as being "fashioned from a red stone, polished like marble."[19]

The material of some of the existing disks, when new, may well have possessed qualities in harmony with Colden's descriptions. In all

[15] Colden, 1902, Vol. 1, pp. 130–131
[16] Colden, 1902, Vol. 1, p. 131.
[17] Colden, 1902, Vol. 1, p. 226.
[18] Colden, 1902, Vol. 1, p. 131.
[19] Kenton, 1927, Vol. 2, p. 271.

likelihood they originally had both a higher polish and a more intense color than they now show; a polish and color subsequently reduced by the chemical action of the earth during a period of burial which, at a minimum, must have lasted over a century. It is also conceivable that, when the stone itself was not of the desired color or texture, artificial coloring was applied. Indeed, the existing traces of paint which led to the theory that these objects were palettes may possibly be vestiges of such an original painted surface.

It is noteworthy in this connection that the traces of paint remaining on the disks are always described as red or white. It seems unlikely that the colors mixed on ceremonial palettes would have been limited in this manner; hence, the exclusive presence of these two colors appears to militate against the theory that they were palettes. On the other hand, the presence of at least the red paint is consistent with the theory that they were "suns." Red is one of the colors most universally associated with the sun and was the actual color of the two stone "suns" reported by Colden. If the existing disks were indeed made to serve as "suns," the presence of red on many of them would be a logical outcome of the desire to make them appear sun-like. Moore's descriptions of a number of the disks he excavated are congruent with such a possibility. "On this disc," he reports in one instance, "was a considerable amount of red pigment."[20] Of nine disks and five fragments found on his second visit to Moundville he says, "On practically all the palettes was paint, sometimes red, sometimes white."[21]

The other facts about paint reported by Moore—the frequent use of white or cream and the presence of red on one side and white on the other side of many disks[22]—present no obvious connection with the present theory. It is conceivable that white, because of its luminosity, may have been regarded by the Indians as another sun color. In the case of the disks reported by Colden, it would appear from the descriptions that the stone itself (possibly catlinite) provided the surface color and that the two sides of the disk were therefore red. If, in other instances, artificial coloring was added, and if both red and white were accepted as sun colors, it is possible that "suns" were painted red on one face and white on the other. Judging from Moore's descriptions, the white and the red do not occur together on the same side of any of the disks. Otherwise it might be supposed that the white was an undercoating intended to provide a smooth surface and to add brilliance and transparency to a red coat superimposed upon it.

It has been suggested that the presence of red and white paint on the disks may in some way reflect the use of red and white by the Muskhogean people to differentiate various subdivisions of the tribes, such as the Red Towns and the White Towns, otherwise known as war towns and peace towns. The statements which I have been able to find regarding this differentiation are so indefinite that I am at a loss to form a definite opinion on this point, but the suggestion should be kept in mind. These tribal subdivisions have been briefly discussed by Swanton.[23]

Turning to other considerations, a fact definitely in line with the present interpretation is revealed by Webb and DeJarnette's observation that some of the existing disks "are drilled with a single hole for suspension."[24] This observation can be significantly linked with Colden's account. His report of the address involving the first "sun" concludes with the statement, "After the Seneka Speaker had done, the Wagunha Presents were hung up in the House, in the sight of the whole assembly."[25] Since the "sun" was one of the presents involved, it was presumably hung up with the others. This suggests that it was drilled for suspension and thus shares an additional characteristic in common with a number of the existing disks.

As another approach to the problem, let us consider the engraved or carved decoration of the existing disks in an effort to determine whether it shows any relevancy to the "sun" theory. By far the most common form of decoration is the notched or scalloped border design. Of the eighteen disks illustrated by Webb and DeJarnette,[26] some of which are reproduced from their plates in our Figure 1, all but one have such borders. On fourteen of these disks the notches are combined with one or more

[20] Moore, 1905, p. 196.
[21] Moore, 1907, p. 392.
[22] Moore, 1905, pp. 204, 206, 235.
[23] Swanton, 1928, pp. 165, 249.
[24] Webb and DeJarnette, 1942, p. 287.
[25] Colden, 1902, Vol. 1, p. 132.
[26] Webb and DeJarnette, 1942, figs. 92–93, pp. 288–289.

concentric circles into a more elaborate border, and on three the notches are combined with engraved surface images.

We have already observed that attempts to connect the notches with a theory of time measurement proved unsuccessful. On the other hand, it is unlikely that the notched border was produced purely by chance or exclusively for decorative effect. Presumably the carvers had *some* definite reason for repeating so many times a pattern requiring such regular spacing and careful execution.

It seems highly probable to the writer that the notched border is a conventional method of representing the sun's rays. If the reader will examine the examples reproduced in our illustrations, I believe he will agree that the border definitely gives a sun-like appearance to many of the disks bearing it. When we look at the sun, its outline usually seems wavering, owing to the intensity of the light falling upon the retina. This gives the impression of a flaming or ray-encircled body rather than a geometrical, circular one. It would be difficult to convey this impression through the conventions of decorative stone-carving more effectively than is accomplished by the notched border designs of many of the disks.

This interpretation, though curiously absent from the published literature on the subject, has not been without supporters. Thus, the National Museum in Washington exhibits specimens of the disks in what it calls a "Synoptic Series of Pigment Plates." The explanation accompanying the exhibit includes this statement: "The margin of the plate is usually scalloped, or notched, and it is surmised that these features represent the rays of the sun, and that the pigments ground upon the plates were used in ceremonies relating to the worship of the sun." Holmes' tentative classification of shell gorgets in the form of disks with scalloped borders as sun circles constitutes a parallel interpretation.[27]

Holmes once proposed another interpretation of the notches, suggesting that they were intended to represent the ends of feathers of the plumed serpent concentrically arranged around the center of the disk. "It is observed," he says, "that the notches cut in the edges of the plates are in many instances carried inward over the plate in such a way as to suggest feathers as these are often formally treated in native art. . . . Recalling the occurrence of the feathered-serpent design on the obverse of the Mississippi tablet a strong presumption is created that the original concept in the mind of the makers of these plates was, at least in some cases, the feathered serpent, a northern form of Quetzalcoatl, a chief deity of the middle American peoples."[28]

The reader may prefer to believe the notches represent the feathers of the plumed serpent, rather than the rays of the sun, without prejudice to the theory that the disks represent treaty "suns." Both interpretations imply the solar significance of the disks, the one in a more symbolical, the other in a more literal, manner. Personally, I do not find in the disks I have seen a confirmation of Holmes' observation that the notches "are in many instances carried inward over the plate in such a way as to suggest feathers." To interpret the notches as representing the sun's rays seems to me more in keeping with the actual designs.

Both interpretations—that the notches represent rays or feathers—provide a ready explanation of the great variety and inconsistency in number and disposition of the notches. As no fixed number of notches would be required to suggest either rays or feathers, each carver would be free to space the notches according to his own imagination and creative impulse. Variety and numerical inconsistency in the notching would inevitably result.

The disks bearing engraved images require special study. The outstanding examples are four in number: the so-called "Mississippi tablet" bearing twin feathered serpents (Fig. 1, *b*, and Pl. I, *d*); the disk with "horned rattlesnakes" encircling an eye-bearing hand (Fig. 1, *e*; the disk with two eye-bearing hands and other figures (Fig. 1, *h*); and the example displaying a large open eye (Fig. 1, *k*). For purpose of reference, I shall call these three latter examples respectively the "single-hand disk," the "twin-hand disk," and the "open-eye disk."

The conception of the images engraved on these disks must certainly have been relevant, in the minds of their makers, to the function of the disks. If we could determine the original meaning of the images, that meaning would presumably either reinforce the present theory or conflict with it. So far as I am able to determine, the authors who have touched at all upon this phase of the subject have felt it neces-

[27] Holmes, 1883, p. 273.

[28] Holmes, 1906, p. 106.

sary to confine themselves almost entirely to a descriptive approach. The principal motifs of the engraving have been classified as the feathered serpent, the hand and eye, the open eye, and the death head. Only in the first instance does this classification give us any assistance in interpreting the designs.

The feathered serpent ascription, based primarily upon the Mississippi tablet (Fig. 1, *b*, and Pl. I, *d*), has the support of Holmes, already quoted, and of various subsequent writers. Among others supporting this interpretation are Douglas and d'Harnoncourt, who remark of the Mississippi tablet, "The disk illustrated here is decorated with two interwined rattlesnakes with feathered heads which suggest Quetzalcoatl, the Plumed Serpent of Mexico."[29]

We have, then, one definite clue as to the interpretation of the engraved designs. The point of special interest for us is whether the mythical concepts associated with the feathered serpent show a positive, a negative, or a neutral relationship to the function we have proposed for the disks. A positive relation appears at once, for students of the subject inform us that Quetzalcoatl was, in the words of Lewis Spence, "a deity of solar significance."[30] " . . . the Man of the Sun . . . . Quetzalcoatl was represented as a traveller with staff in hand, and this is proof of his solar character, as is the statement that under his rule the fruits of the earth flourished more abundantly than at any subsequent period. The abundance of gold said to have been accumulated in his reign assists the theory, the precious metal being invariably associated with the sun by most barbarous peoples. In the native *pinturas* it is noticeable that the solar disk and semi-disk are almost invariably found in connection with the feathered serpent as the symbolical attributes of Quetzalcoatl. The Hopi Indians of Arizona at the present day symbolize the sun as a serpent, tail in mouth, and the ancient Mexicans introduced the solar disk in connection with small images of Quetzalcoatl, which they attached to the head-dress. In still other examples, Quetzalcoatl is pictured as if emerging or stepping from the luminary, which is represented as his dwelling place."[31] "In the reign of Quetzalcoatl," says another passage, "there was peace and plenty for all men."[32]

All this is definitely relevant to the theory that the disks were "suns" and to the employment of the "suns" in connection with pledges of peace and with the visions of fulfillment and plenty associated with peace. Had the carver of the Mississippi tablet wished to select a mythical—that is to say, for him, religious—image expressive of concepts related to the purpose of a treaty "sun," he could not have chosen a form more suitable than that of Quetzalcoatl or some related variation of the feathered serpent.

Why the design comprises *two* feathered serpents is a matter for conjecture. From the decorative point of view the repeated image facilitates an attractive symmetry, but we may presume that the engraver had more than a decorative end in view. It will be noted that twin elements occur a number of times in the engraved designs: twin serpents in two of them, twin heads and hands in a third. In the latter instance, they are not so disposed as to provide the design with symmetry and must therefore have resulted from other than a decorative impulse.

The treaty context under consideration suggests the possibility that these double representations were intended to symbolize the joint adherence of the two separate parties to the agreement. In these terms the iconography of the Mississippi tablet would be a perfect embodiment of the concepts associated with a treaty "sun." The presence of the feathered serpent heightens the solar, and therefore the sacred, significance of the signatory object; the doubling and intertwining of the images symbolizes the intermingling of the spirits of two peoples in a common will to peace.

The designs on the other three engraved disks can only be hypothetically interpreted. To my knowledge no attempt has yet been made to interpret the meaning of the hand-and-eye, the open-eye, or the death-head motifs, nor has a name even been attached to the scepter-like and the eccentric forms which complete the imagery of the twin-hand disk. In the absence of specific information regarding the significance of these several motifs, let us examine them in the light of the treaty "sun" concept and attempt to determine whether possible meanings which are in harmony with the "sun" concept, with the observable characteristics of the engraved imagery, and with what we know of Indian mentality in general suggest themselves.

For such a purpose, the question of mentality

[29] Douglas and d'Harnoncourt, 1941, p. 87.
[30] Spence, N. D., p. 21.
[31] Spence, N. D., pp. 81–82.
[32] Spence, N. D., p. 60.

is of great importance. The ultimate source and explanation of every artifact is a mental one. The substance and structural potentialities of the object are derived from the material world, but its essential character and significance are determined by the impulses and attainments of the human psyche at a certain stage of cultural evolution. And just as artifacts embody and preserve for us various levels of collective psychic experience, so also whatever we can learn about any level of psychic experience illuminates the artifacts which emerged from that level.

Applying this principle to the present problem, we find that early observers of the Indians describe certain mental attitudes and traditions which are of considerable moment to us. I shall quote two descriptions of these attitudes as observed among the Hurons and recorded in the Jesuit Relations. The first is from the Relation of 1636 by Father Paul le Jeune. "They have recourse to the Sky in almost all their necessities, and respect the great bodies in it above all creations, and remark in it in particular something divine. . . . They even fear his anger, and invoke him as a witness in order to render their faith inviolable, when they make some promise of importance, or agree to some bargain or treaty of peace with an enemy. Here are the terms they use, *Hakhrihôtê ekaronhiatê tout Icwakhier ekentatê*, 'The Sky knows what we are doing today,' and they think that if, after this, they should violate their word or break their alliance, the Sky would certainly chastise them."[33]

Father Paul Ragueneau makes a similar observation in the Relation of 1647–1648. "Very frequently they address themselves to the Sky, paying it homage; and they call upon the Sun to be witness of their courage, of their misery, or of their innocence. But, above all, in treaties of peace and alliance with foreign Nations they invoke, as witness of their sincerity, the Sun and the Sky, which see into the depths of their hearts, and will wreak vengeance on the treachery of those who betray their trust and do not keep their word."[34]

Similar concepts prevailed widely among the Indians and among other peoples in a corresponding stage of mental development. It will be noted that these concepts involve the idea of an all-penetrating supernatural vision which is capable of overseeing the affairs of men, and an all-governing supernatural power which is capable of punishing violations of faith; further, that this vision and power are associated in the Indian mind with imagery provided by the sky and the sun.

When we re-examine the motifs engraved on the disks in the light of these ideas, it becomes apparent that the former might easily have been symbolical expressions of the latter. The open eye is one of the most obvious and most effective symbols of an all-seeing supernatural vision, and has been so used in later religious contexts as well as in connection with sun worship. The hand and eye combines this concept of all-seeing vision with that of the all-governing power: a strong hand ready to punish the unfaithful. The death head could conceivably symbolize the vengeance to be wreaked upon those who betray their obligations. Moore illustrates a border design, engraved on a bowl, which consists of eye-bearing hands alternating with death heads.[35] Whether or not it was so intended, such a design would at least make sense if we conceived of it as representing three related aspects of the psychic state we have been considering. As such, it would simply be a pictographic statement of the consciousness, "We are seen, we are ruled, we may be punished."

Acceptance of these suggestions provides a basis for a plausible interpretation of the three engraved designs under discussion, an interpretation consistent with all the knowledge we possess of the total psycho-historical-archaeological context from which the disks emerged. Taking first the design on what we have called the single-hand disk (Fig. 1, *e*), the supernatural vision and power of the sun are symbolized in the hand-and-eye motif. The two "horned" rattlesnakes are presumably somewhat crude representations of the feathered serpent, the "horns" being abbreviated indications of the serpent's plumes. The presence of two snakes and the fact that they are knotted together seem to give even more definite expression to an idea suggested above in connection with the Mississippi tablet; the idea, namely, of binding together the spirits of two peoples. The fact that the serpents are rattlesnakes, and the emphasis given to the rattles on both this disk and the Mississippi tablet, might well symbolize the element of warning which each party to a treaty usually wishes to impress upon the other.

[33] Kenton, 1927, Vol. 1, pp. 261–262.
[34] Kenton, 1927, Vol. 1, pp. 515–516.
[35] Moore, 1905, p. 175.

The design on the open-eye disk (Fig. 1, *k*) is easily interpreted in terms of the ideas being considered. Here the designer has concentrated on one large symbol of the all-seeing vision, leaving the other elements of the context to function as an implied mental background.

The twin-hand disk (Fig. 1, *h*) can be only partially deciphered in terms of the present ideas. Enough has already been said to indicate the presumed meaning of the twin eye-bearing hands, but the other elements of the design evade satisfactory interpretation. The central bar with the twin heads might possibly be a simplified and condensed rendering of the twin feathered-serpent motif, the use of death heads perhaps emphasizing the idea of avenging power. What I have called the scepter-like form and the eccentric form on this disk remain completely indecipherable in terms of any concepts with which I am familiar.

Taken as a whole, the four disks with their engraved designs appear to lend themselves with considerable readiness and plausibility to the function we have postulated—that of treaty "suns"—and this function in turn provides a mental background in terms of which the designs could easily have been conceived and to which they would seem logically related.

One other fact deserves consideration before this discussion is concluded. The "suns" reported by Colden date from the seventeenth century, as do also the records of Indian mentality quoted from the Jesuit fathers. It would have at least an indirect bearing upon our case if we could determine the probable age of the existing disks, for, if they dated from a much earlier epoch, unity of their cultural background and that of Colden's "suns" would be more problematical than if they came from approximately the same period.

While we have no way of knowing how early such disks may first have been made, two recent studies associate many, if not all, of the existing specimens with a cultural complex involving Middle American influence and infused into southeastern North America at a relatively late date. Among the many aspects associated by archaeologists with this complex are not only examples of the disks themselves, but the use of the bird-serpent motif, the hand-and-eye motif, the open-eye motif, and the death-head motif. The motifs mentioned are used on other objects as well as the disks, and the complex, of course, includes many additional elements not found on the disks. With regard to the chronological position of this complex, Philip Phillips concludes that "there are strong indications that it will fall into the period immediately preceding the historic 'complexes.' . . . In other words, Middle Mississippi seems to belong to the last phase of the pre-Columbian history of the Mississippi Valley, say roughly the interval between 1400 and 1700 A.D. Any connection it may have had with Middle America must be relegated, therefore, to this disgustingly late period."[36]

Waring and Holder have arrived at a similar conclusion. "To summarize the chronological aspects of the complex: these elements appear suddenly and late. When they appear, Macon excepted, they are apparently fully elaborated. No developmental sequences of the objects are traceable."[37]

Thus, chronological considerations in turn seem to favor our theory. The complex with which the disks are associated is assigned to a period which extended roughly from the fifteenth through the seventeenth centuries and therefore included the period during which Colden's "suns" were observed in use and during which the Jesuit fathers made their observations. The possibility that the function of the disks and the "suns" was identical is accordingly strengthened.

CONCLUSION

While positive proof is hardly possible, there is a considerable body of convergent evidence to suggest that the stone disks of the southeastern Indians, usually regarded as "palettes," may have been signatory "suns" connected with peace treaties. Colden provides two historical instances of the use of similar disks for this purpose, whereas no documentary evidence is available in support of any of the previous conjectures as to the function of the disks. The shape, size, and drilling of the existing disks correspond closely to those which Colden describes. The material of the existing disks, the traces of paint found on them, the limitation of the paint to two colors—red and white—and the prominence given to red, are all compatible with, and in some cases favorable to, the present theory. The presence of the feathered serpent among the images engraved on the disks provides definite proof of a solar association. The notching and the use of the hand-and-eye and open-eye motifs also lend

[36] Phillips, f 1940, p. 365.
[37] Waring and Holder, 1945, p. 28.

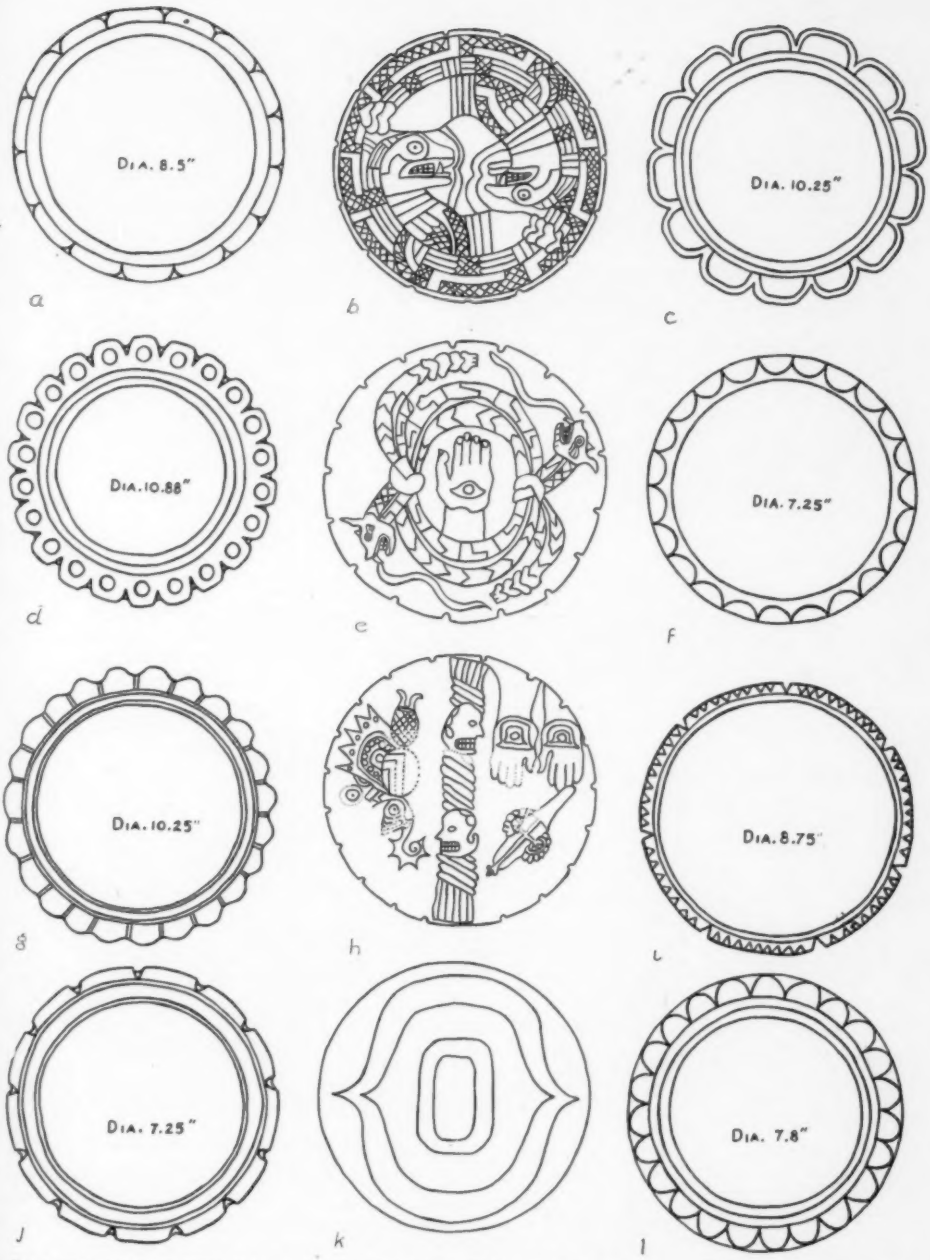
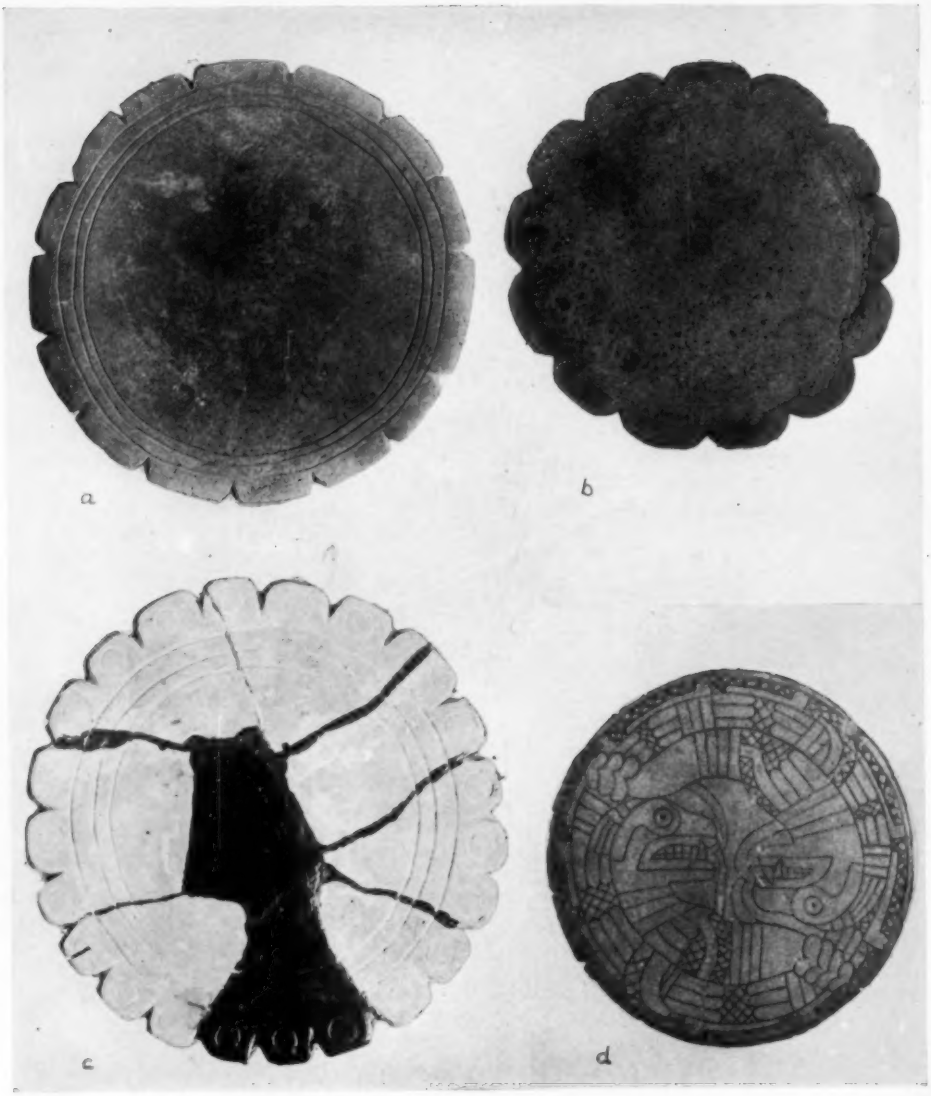


FIG. 1.—Southeastern stone disks: *a*, *c*, *d*, *f*, *g*, *i*, *j*, *l*, after Webb and DeJarnette; *b*, the "Mississippi Tablet," in the Ohio State Museum, from a mound in Issaquena County, Mississippi; *e*, disk with two "horned rattlesnakes" surrounding an eye-bearing hand; *h*, disk with two eye-bearing hands and other figures; *k*, the "open eye" disk. *e*, *h*, and *k* are from Moundville, Alabama.



Southeastern stone disks:

a, *c*, disks from Moundville, Alabama, in the Alabama Museum of Natural History; *b*, disk from Moundville, Alabama, in the Museum of the American Indian, New York; *d*, disk from a mound in Issaquena Co., Mississippi, the so-called "Mississippi Tablet," now in the Ohio State Museum. All photos courtesy of the institutions mentioned, except for *d*, which is courtesy of the American Museum of Natural History, New York.

themselves readily to a solar interpretation. The cultural complex with which the disks are associated is ascribed by archaeologists to comparatively recent centuries, including the century of Colden's recorded examples.

In view of all these facts, it seems safe to conclude that there is at least as much evidence to support the theory that the disks were treaty suns as there is to support the theory that they were palettes.

Whether the disks and the rectangular tablets, at present grouped together as "palettes," are to be differentiated from each other, the former being classed as "suns" and the latter retaining their status as palettes, is a question which I shall not attempt to answer. If the subject is of sufficient interest, perhaps this and other problems arising out of the theory presented here will be taken up at some time by readers of this paper.

Exhaustive search through the historical records of Indian affairs would no doubt bring to light further instances of the use of stone disks as signatory objects. My own incidental reading, carried on for a different purpose, has furnished one additional example of the use of a signatory "sun," though in this case the latter was made of beads, not stone. This instance is recorded by Father Jean de Quen in the Jesuit Relation for the year 1655–1656: "In the summer of last year, 1655, it was thought necessary to send a Father of our Society into the country of the Agnieronon Iroquois [Mohawks], in order that we might, by this show of friendship and confidence, confirm the peace with them."[38] The emissary reached the Mohawks, made his peace overtures, and displayed the presents which he had brought from the French governor. "One of the Iroquois Captains exhibited, in his turn, some very rich presents, in answer to the various articles of peace proposed by the Father. The first and finest of these presents was a large image of the Sun, made of six thousand porcelain beads, —its purpose being, as he said, to dispel all darkness from our councils, and to let the Sun illumine them even in the deepest gloom of night."[39]

[38] Kenton, 1927, Vol. 2, p. 62.
[39] Kenton, 1927, Vol. 2, p. 63.

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PREHISTORIC CULTURES OF CUBA

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IN RECENT years a considerable amount of archaeological research has contributed to our knowledge of the Antilles. Especially valuable in this field are the numerous works of Osgood,[1] Rouse,[2] and Rainey,[3] all of the Peabody Museum of Yale University, whose valuable investigations and excellent monographs have brought new life to the archaeology of the Antilles.

Nevertheless, much remains to be done, and important discoveries relating to Antillean problems will probably yet be made. Cultural relationships between the Antilles and the two adjacent continents constitute one of the primary historical problems of great importance. The affiliations of the Antillean cultures cannot be established until the archaeology of such points of possible contact as Venezuela, Yucatan, and Florida is more fully known.[4] Data on the Floridian peoples and their cultures have been particularly scant, but recently evidence has been accumulating in support of the view that there were prehistoric contacts between the Antilles and the United States and Mexico. Something of the archaeology of Venezuela is also beginning to be known from the work of the Peabody Museum in that country. Trading connections existed between Yucatan and Cuba, but the diffusion of cultures seems to have been negligible.

Furthermore, in order for the archaeology of Cuba to be completely known we must establish its natural connections with the other Antillean islands by studying the distribution of the cultural traits within carefully defined geographical areas. Unfortunately, it is difficult to obtain evidence of radiating diffusion in the Antilles because they are a chain of islands rather than a single land mass.

Fundamentally, there are two well-defined cultural horizons in the Antilles; the first an archaic one lacking ceramics and agriculture and with implements of shell or stone made in a paleolithic technology, the second marked by agriculture and ceramics and some polished stone artifacts. Of course, it must be taken into account that these primitive horizons cannot have come into existence at specific historic moments but, on the contrary, must have developed continuously over many years and possibly some centuries, as is shown by the differences found in the horizons.

[1] Osgood, 1942*a*, 1942*b*, 1943; Osgood and Howard, 1943.
[2] Rouse, 1937, 1939, 1940, 1941, 1942.
[3] Rainey, 1935, 1941. [4] Gower, 1927.

ARCHAIC CULTURE OF THE ANTILLES

The earliest migration must have been small in size and its progress must have been very slow on account of the means of transportation available to the men of the archaic culture. Only by jumping from island to island, we may say, did the migrants finally reach Cuba.

The first settlers of Cuba brought the knowledge of fire and the ability to produce it, a rough and very primitive shell culture, utensils of natural stones, and also, it is speculated, a rudimentary system of religious beliefs founded on magic and dread of or respect for the dead.

The first groups of people must have been intensely influenced by external factors, such as the climate, their geographic position, and the structure of the land. These shaped their economic action, limiting their diet to products easily acquired locally and affecting their division of work in the search for food. Local resources without any doubt were predominant in the physical and moral life of the individual and of his incipient communities.

Their dependence on food which could be simply collected along sheltered coasts and in shallow waters bound the Indians to the shore and forced them to move as the food supply in any sector became exhausted. This dependence upon an unstable food supply fixed in the habits of the people, as a stamp of rudimentary culture, a seminomadism that provided them only with a greater liberty of movement.

All of the other geographic elements combined with the organic resources of the various Antillean islands to make the first occupant a true "shore-man," a collector of the products of the sea more than a fisherman or a hunter, although he did not disdain easily captured animals such as the Cuban ground sloths (*Megalocnus*) of the Pleistocene, the remains of which often appear in the sites of this primitive man with handiworks of his rudimentary culture (Pl. II, *a*).

In the time scale established by Rouse[5] for Puerto Rico and Haiti, this archaic culture has been assigned to Period I. Not only does the archaic culture lack ceramics and agriculture, but it also has other primitive characteristics: the small size of the groups, their collecting economy and seminomadic life, and their custom of living at camp sites in open country resulted in an absence of permanent villages; the paleolithic techniques which they employed produced easily recognizable artifacts.

The distribution of this archaic culture during Period I in the Antilles includes Cuba, the Haitian part of the island of Hispaniola, Puerto Rico, and the Virgin Islands. Thus far, the culture that characterizes this period has not been found in the Bahama Islands, the Santo Domingan part of Hispaniola, Jamaica, or the Lesser Antilles, although several of its traits have appeared on the coastal plain of the Venezuelan province of Barcelona.[6]

The characteristics of this culture may be summarized as follows:

1. Camp sites in the open; these consist of small shell middens on or near the coast.
2. The contents of the shell middens always consist of shells, bones of animals, and remains of fish—that is to say, actual remains of food—together with implements and other objects of the people's industry.
3. The technology was paleolithic, in lithic material or in shell. Also found in the middens are fragments of ochre or hematite, used for red paint, and limonite, for yellow paint.

In the Antilles, during Period I, we can establish two cultural areas—a western one that comprises Cuba and Haiti, and an eastern area that comprises Puerto Rico and the Virgin Islands. They are so different that they can be considered to have distinct origins, the first in Florida, the second on the coast of South America.

The Guanajatabey of Cuba

We have named the natives who were in possession of this archaic culture in Cuba "Guanajatabey," a term used in the old chronicles of the conquest of Cuba by Oviedo, Herrera, Las Casas, and Pedro Martyr de Anglería, as well as in documents of the epoch, to refer to the sylvatic Indians who occupied the extreme western parts of the provinces of Pinar del Río in Cuba and of Guacayarima in Haiti at the time of the discovery and conquest.

Father Las Casas[7] described the Guanajatabey as follows: "Some Indians at the end of Cuba, who are like savages, that in no way treated with those of the island (Cuba), nor they have houses, they are continuously in caves, unless when they go out to fish." Diego Velázquez,[8] when he informed the King of Spain in 1511 of the conquest of Cuba, said, "There are two Provinces in the western part of Cuba, and one of these is called Guaniguanico, the other, Guanahabibes. The latter is situated at the western extremity, where the natives lived as savages, having neither houses nor farms, subsisting on game, captured in the mountains, or on turtles and fishes."

These accounts, and the report made by Columbus during his second voyage along the southern coast of Cuba, lead us to apply the name Guanajatabey to the archaic Indians, whose language, the "Lucayo," Columbus' interpreter Diego could not understand. Guanajatabey is the proper name for those Indians rather than Ciboney, the name erroneously given to them by Harrington.[9]

The Guanajatabey culture cannot be homogeneous nor uniform in time and space, precisely because of its archaic character. It is very old in Cuban territory. Its distribution over the whole sea shore, in places where food is available for gathering, is well demonstrated. Association of Guanajatabey remains with bones of animals belonging to the Pleistocene fauna, extinct at the time of the discovery, such as the *Megalocnus*, *Nesophantes micrus*, *Boromys*, and other smaller species has been frequently noted. This association demonstrates the great antiquity of the culture.

The Cuban Guanajatabey culture as a whole embraces three distinctive aspects which, in order of antiquity and in terms of principal places of occurrence, can be listed as:

1. Guayabo Blanco (Cienaga de Zapata, Santa Clara).
2. Cayo Redondo (Guanacahabibes Peninsula, Pinar del Río).
3. Maniabón, Banes, Oriente, and many other places.

Culture of Guayabo Blanco.—The objects and implements of this aspect, the most archaic of all, were found principally in the "funerary mound" of Guayabo Blanco by the writer in 1913 and comprise an industry typically of shells—gouges, cups, tips, and plates.[10] With

[5] Rouse, 1939, 1942. [6] Osgood, and Howard, 1943. [7] Las Casas, 1909. [8] Navarrete, 1825–1837. [9] Harrington, 1921. [10] Cosculluela, 1918.

the exception of a very small amount of rough lithic material, such as hammers composed of natural stones of sufficient weight and volume for man's purpose, the implements were all made from shells, especially from those of the conch *Strombus gigas* (Pl. II, *b*).

Culture of Cayo Redondo.—A more evolved aspect of the Guanajatabey culture was discovered by Professor Osgood in 1941 at the place of residence of the Guanajatabey during the Ciboney and Taino epochs.[11] Objects and implements shown on Plate II, *c* and *d* are characteristic of this culture.

The specimens of stone are numerous: picks, hammer-grinder, polishing stone, mortars, milling stones, sharpening stone, stone balls, hand ax, and a ceremonial stone. Among them, the most notable is the ceremonial stone, shown on Plate II, *d*. This has two ear-like protuberances, and resembles similar objects found in the Lesser Antilles and subsequently discovered by several investigators in other parts of Cuba, within the area of distribution of the Guanajatabey culture.

Although Osgood's collection from Cayo Redondo includes some shell tools, like the typical gouge, the greater number of the industrial implements were made of stone, in contrast to the situation in the Guayabo Blanco aspect.

Culture of Maniabón (Banes).—In the year 1941, Rouse located a great number of camp sites of the Cuban Guanajatabey along the sea shore of Oriente Province. In content these shell middens are very similar to those found by Harrington.[12] According to Rouse, camp sites in the open have an interesting distribution. All are on the coast. With the exception of one site on the peninsula of Ramón and two on the bay of Manatí, they are concentrated in a small area in the north-central part of the Maniabón Hills. Six are clustered around an inlet. It is noteworthy that the sites clustered together are extremely close, never more than 150 meters apart.[13]

Pieces of flint are the commonest materials at the sites found by Rouse; traces of ochre, probably used for paint, have been found in two places, and pieces of coral, the shapes of which suggest rasps, in six. There are fifteen hammerstones, the majority of which are irregular pebbles. A hammer-grinder, stone balls, a hand ax, and an undrilled pendant of a simple type complete the list of stone artifacts. The remaining objects are of conch shell and include gouges, blades, lip, plates, and tips, but the relative rarity of gouges and the absence of cups is surprising.

[11] Osgood, 1942*a*. [12] Harrington, 1921.
[13] Rouse, 1942, p. 131.

Differences in the three aspects of the Cuban Guanajatabey

It is logical to assume that the three aspects of the Cuban Guanajatabey culture were not contemporary; on the contrary, one must have succeeded the other. When studied in detail, each one can be assigned on the basis of its characteristics to a certain time or epoch.

The culture of Guayabo Blanco is typically a culture of shell; the man who possessed it could not use stone as the material for his implements, for it was more difficult to work than was shell. The cultures of Cayo Redondo and Maniabón, which followed, are typical lithic cultures, since implements of stone predominate and there are very few artifacts of conch. This fact represents, indeed, a cultural progress which modern man cannot duly appreciate. The stone implements of Cayo Redondo are very crude, without any artistic sense. Only a rudimentary principle of symmetry can be found in them, but they are becoming better in Maniabón.

In the culture of Guayabo Blanco there cannot be found any stone balls, stone hand axes, mortars, grinders, or picks; that is to say, all the instruments necessary to work stone are absent. Thus, it is a typical shell culture. Cayo Redondo and Maniabón represent a more advanced aspect of the same culture, although not with the development in the stone industry that came about in Haiti and which is described below.

Objects of personal ornamentation, like beads, appeared principally in the last aspect of Maniabón. There are also stone balls, perhaps of ceremonial significance, in both the Cayo Redondo and Maniabón cultures, but they are absent in Guayabo Blanco, the most archaic of them all.

The existence of paint indicates possibilities beyond the basic pattern of covering surfaces, especially in woods. The baton of wood found by Harrington in Lake Malpoton in Pinar del Río and illustrated in Plate II, *e*, is one of the few recorded examples of the decorative art of the more primitive people which might have

been painted.[14] The presence of red and yellow paint in the sites points to its use for coloring the face or body as personal adornment.

At Cayo Redondo nothing was recovered to indicate that the people had acquired artifacts from other people. In other words, no trade could be demonstrated.

In cultures similar to Maniabón at the camp site of Monte Cristo, described by Harrington,[15] are some specimens of flint more elaborate than those of Cayo Redondo; a knife, a scraper, and a tip of an arrow reveal the progress in the lithic industry of the Guanajatabey in Cuba. There is also a very good shell ax of almond shape from the Loma del Rizo.

Relations with Haiti

In Haiti, the excavations of Rainey and Rouse[16] have revealed the existence of a somewhat more advanced culture, the Courí, which is characterized principally by three types of flint artifacts—daggers, knives, and scrapers. The typical shell gouge of the Cuban Guanajatabey does not appear; substituted for it is the stone hatchet. The reason that the Courí culture is much more advanced than that of Cuba is simple; the Cuban Guanajatabey lived for centuries alone, isolated without any exterior contact, as is demonstrated by the absence of intrusive forms among their implements and utilitarian objects, while the Haitians maintained a continuous trade with the peoples who occupied the chain of smaller islands connecting their homeland with the coast of Venezuela and South America.

The Archaic Man of Guayabo Blanco

The mound burials at Guayabo Blanco provide information on the people who possessed the most primitive culture of Cuba, showing that they had somatic characteristics, as well as cultural features, which distinguish them from the peaceful Arawak who in Period III invaded the Antilles. The Guanajatabey culture is very old in Cuba, but evidence of the occupation of the island before the Christian era is very uncertain.

Head deformation was not practiced by the Guanajatabey, as Plate II *f* shows. The skull was normal, sub-brachycephalic in form, and very massive, with great facial development and a very pronounced lower jaw; all this indicates a man of considerable height and a strong and healthy constitution, as might be expected in view of his phosphate-rich diet of molluscs.

All of the skulls have certain very prominent and conspicuous characteristics which give them a pronounced individuality; they are very massive and the bony matter is extremely dense. None of the skulls are of individuals who had lived to very old age.

Survival of the Guanajatabey in Cuba

The Spanish conquerors found the Cuban Guanajatabey in the western parts of the island, where they had taken refuge at the beginning of Ciboney hegemony. During the whole of Period II in the Antilles, marked by the appearance of ceramics in Puerto Rico with the culture that Rainey called "Crab Culture,"[17] the Guanajatabey had continued their tranquil backward life in Cuba. The new culture of Puerto Rico did not pass to Haiti and Cuba until the beginning of Period III, at which time the Guanajatabey were driven into western Cuba.

The Guanajatabey survived until the colonization of the extreme western part of the island. The first historic reference we have to their language is the statement in Columbus' account of his second voyage along the southern coast of Cuba that his Lucayan interpreter, Diego Colón, could not understand the language of the Indians of the region near Batabanó, as they spoke a different dialect. Father Las Casas' statement that in Cuba there were three languages[18] perhaps refers to the three types of Indians of the island, one of which is the Guanajatabey.

THE AGRICULTURAL TRIBES OF THE ANTILLES

In the extensive region bordering the Caribbean Sea in Venezuela and Colombia, there lived for many centuries a tribe known as the Nuarhuaco, or Arawak. Their language, already lost at the time of the discovery, gave origin to numerous branches, among them the Achagua, possible ancestor of the Cuban Ciboney. With time, these dialects became more dissimilar, until it is now difficult to correlate them idiomatically. The numerous tribes which the Spanish found occupying that region had no more in common than their Arawak origin.[19]

The expansion and migration of the Arawak population toward the Antilles took place in a very remote epoch, as is shown by the profound

[14] Cosculluela, 1944.
[15] Rainey, 1941; Rouse, 1941.
[16] Rainey, 1935.
[17] Rainey, 1935.
[18] Las Casas, 1909.
[19] Cosculluela, 1943.

idiomatic differences among their dialects which could only be effected after numerous centuries of separation and tribal isolation. The Arawak families must have moved through the Lesser and Greater Antilles until they reached Cuba, the last island occupied, because of its position at the extreme end of the route.

In the Lesser Antilles these Arawak people gave origin to the Ygneri culture by a series of waves of migration starting from South America during the former Period II. In Puerto Rico the people of the second period were the bearers of the culture that Rainey has called "Crab."[20] They colonized Hispaniola, Cuba, and Jamaica during Period III, introducing agriculture based on the cultivation of manioc. Their culture is marked, at the beginning of Period III, by the ceramics which Rouse called "Meillac" in Haiti and "Collores" in Puerto Rico and the Virgin Islands.[21]

This wave of Arawak agricultural tribes covered the whole of the Antilles, Lesser and Greater. Furthermore, Barbados, which at the time of the discovery was uninhabited, previously supported a large Arawak population, as is demonstrated by archaeological investigations.[22] Jamaica, relatively separated from the route followed, was also occupied, along with the other islands from Florida to the coast of Venezuela.

The Arawak world not only covered all the territory of the Antilles, but also extended deeply into the heart of Venezuela and Columbia. Trade between all the islands and the continent was very intensive before the Carib invasion of the Lesser Antilles.

Cultural characteristics of the Arawak people in the Greater Antilles are as follows:

1. The village sites contain ceramics and, among them, the remains of "burens," indicating an agriculture dedicated mainly to the raising of yuca and the preparation of cassava.

2. Because of this agricultural economy, villages were situated in regions favorable for the cultivation of yuca and cassava.

3. By their agricultural economy the people were attached to the soil. They had real villages, of 1,000 to 5,000 inhabitants. The houses, circular or polygonal, were not placed in alignment, and there were no open places or plazas.

4. Objects and implements of flint are not common in this culture. The small ax or hatchet of stone is absent, and substituted for it in a general and uniform way is the "petaloid" celt. Numerous objects and implements of bone and shell are also included among the implements.

5. Head deformation—fronto-occipital flattening—was practiced.

[20] Rainey, 1935. [21] Rouse, 1941. [22] Fewkes, 1922.

The Ciboney People of Cuba

In about the twelfth or the thirteenth century of our era, Rouse has estimated from the depth of some middens in Banes, Arawak appear in possession of the eastern part of Cuba.[23] We call these families of farmers "Ciboney,"[24] on the basis of historical evidence contained in old Spanish Chronicles of the Indies and numerous documents of the Archives of the Indies.[25] In all the documents, these people are designated as Ciboney.

Father Las Casas said, "The natives of this island [Cuba] call themselves in their language, 'Siboneyes,' [and] 'their natives [those of Cuba] were not known in Haiti with another name than that of 'Siboneyes.' "[26] In old documents of the Archives of the Indies it is said of the Indians of Cuba: "Others [Indians] there are called 'Ciboneyes,' that the Indians of the same island [the Taino] have as servants, and so are all of said Gardens (islets at the south of Camagüey)."[27]

Harrington, in *Cuba before Columbus*,[28] wrote: "We may at least call attention to the fact that to Sr. Cosculluela belongs the credit of first publishing, in unmistakable terms, the true significance of the name Ciboney, hitherto generally accepted, particularly in Cuba, as a generic term for all the Cuban Indians, as typified by the Taino inhabiting most of the island when colonization began." At that time, I applied the name to the earliest Indians, but I am now sure that I was mistaken and that the name "Ciboney" belongs to the first Arawak people who came to Cuba, not to the old Guanajatabey.[29]

So we will call "Ciboney" the people who first appeared in Cuba with ceramics and a knowledge of agriculture, and who came to occupy the island only east of the meridian of the Bay of Xagua in Cienfuegos, where so far there have appeared only village sites of their culture.[30]

Since the time of Harrington, all sites similar to those found in Maniabón, Oriente, by Rouse[31] have been considered Taino. "There are two main reasons," says Rouse, "for

[23] Rouse, 1942. [24] Pichardo Moya, 1934, 1935.
[25] Ortiz, 1943. [26] Las Casas, 1909.
[27] Navarrete, 1825–1837. [28] Harrington, 1921.
[29] Ortiz, 1943.
[30] Morales Patiño, 1943. [31] Rouse, 1942.

identifying them instead with the sub-Taino Indians [whom we call Ciboney]. First, the earliest middens in these sites differ in culture from the middens in the Taino sites, as known in Baracoa on the eastern tip of Cuba, and resemble instead several non-Taino sites in that region. They can also be correlated with the non-Taino Meillac group of sites in Haiti, and with the comparable group of sites in Jamaica, which Harrington has called sub-Taino."[32] "The second reason," he continues, "is the depth of the sites, which demonstrates that they were settled long before the year 1460 when the migration of Tainos from Hispaniola to Cuba is said by Las Casas[33] to have taken place."

Ethnological and philological data point to the fact that the Ciboney proceed from the Achaguas branch of Arawak, while the Taino seem to have closer affinities with the Guagiro. The Ciboney culture is much older than the Taino, and in many aspects in Cuba superior to it, but we cannot forget that the Taino in Cuba did not colonize any land other than the extreme eastern region of Baracoa and never, in Indian times, passed to the west. Up to the present time, Taino village sites are not known away from that extreme eastern part of the island.[34]

The contents of the Ciboney culture found in Maniabón are illustrated in Plate II *g*, *h*. That the Ciboney in Cuba adopted many elements of the culture of their predecessors, the Guanajatabey, is indicated by the presence in the Ciboney sites of all but one of the Guanajatabey types of artifacts, and perhaps also by the fact that the Ciboney possessed, in Cuba, the principal Guanajatabey type of site. Rouse says that the Ciboney emphasis on shell work, a feature which distinguishes the Cuban Ciboney from the Ciboney of other islands can probably be attributed to Guanajatabey influence.[35]

"By far the largest number of aboriginal artifacts are potsherds; there are also a number of whole pots, more, probably, than in the rest of the West Indies combined. The pottery seems to be of two kinds: bowls and platters. The bowls were probably used as cooking vessels, since many of them bear traces of soot. Most are round, some are boat shaped. . . .

"All bowls are not decorated. On those which are, the designs are confined to the shoulder and rim.

"It would appear that the earlier pottery in Maniabón Hills lacked curved incised lines and, in particular, the ovoid design. Fragments of griddles, called burens by the Indians, are not uncommon. Net-sinkers of clay have been reported. . . . Several beads and pendants have been made from the same material. There are a number of clay idols, most of which represent women with prominent sexual organs." To these Columbus referred in his journal of the first voyage along the north coast of Cuba. "Many fragments of flint have been found and pieces of quartz are mentioned for some sites and a quartz arrowhead for another."[36]

The commonest type of stone implement is the petaloid stone celt; chisels have been found and stone hammers are widespread. Beads are the most numerous stone ornaments, and pendants have also been cut out of stone, some carved to represent the human face or the male figure. Stone masks, small idols or "zemis" of stone, large idols made from coral, stone balls or "esferolitias," and stone disks have been reported. Bone artifacts are rare.[37]

The Ciboney Indian

The Ciboney of the Maniabón region in the epoch of the discovery is described by Columbus, in his Journal of the first voyage: " . . . naked as his mother gave him birth, well formed of body, and of face, regular height, rather thin, black hair, abundant and crinite, almost like mane of a horse tail, short and with a ring of hair, on his forehead, save a few that wear it long and never cut it. . . . All painted, some of white, others of red, some only the face, others the eyes or the nose, and many the whole body. In general, they have big eyes and agreeable face, the hands thin, small feet, and the body without belly, and with straight legs, and the skin coppery, neither white nor black."

But what attracted most the attention of Columbus, as he says, is the skull: " . . . of narrow forehead and slanting backwards," due to the cranial frontal deformation.

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[32] Harrington, 1921.
[33] Las Casas, 1909.
[34] Cosculluela, 1943.
[35] Rouse, 1942, p. 152. He uses the term "sub-Taino" for "Ciboney" and "Ciboney" for "Guanajatabey." We have substituted the terms dictated by usage in this article.
[36] *Ibid.*, p. 140 *et passim*.
[37] Rouse, 1942.

Comparison of Ciboney Culture of Cuba and that of Haiti

The Ciboney culture of Cuba is very similar to the Meillac of Haiti; the pottery is of the same general kind, although the Meillac ceramics show an emphasis upon incision, the Cuban upon affixation, and there are differences in details. The Meillac culture lacks the carved shell work which is so characteristic of the Cuban Ciboney. The similarities, on the other hand, are so great that, according to Rouse,[38] we may attribute both the Cuban and Haitian types of culture to the Sub-Taino (Ciboney) Indians.

The Cuban Ciboney culture contains a great number of shell implements which do not appear in Haiti, and that fact possibly demonstrates a major contact with the Guanajatabey. It is very significant in this connection that all but one of the types of artifacts of the Guanajatabey culture persisted during the Ciboney period, which indicates, above all in Cuba, a major interchange of culture between the old native occupants and the new, more advanced settlers.[39]

The correlation of Period III in Puerto Rico and Haiti, based on the pottery, without any doubt appears perfect, but in the remaining artifacts there are some important differences. Apart from the ceramics, many of the artifacts of Cuba and of Haiti and Jamaica are of shell. In Puerto Rico the implements of flint so current in the western area do not appear, although the island has the finest work in stone—the so-called "stone collars" (*collares de piedra*).

THE TAINAN INVASION OF THE GREATER ANTILLES

As soon as the Arawak-Carib war commenced, tribes of the first families emigrated from the continent (Venezuela) and occupied some of the Greater Antilles, where tribes of the same origin as the Ciboney of Cuba had been established during many centuries. We know now that the Calina, a branch of the Carib tribe of the continent, occupied the Lesser Antilles and supplanted the earlier agricultural Ygneri population, and that the Carib invaders took wives from among the conquered Ygneri and killed all the men. Probably this invasion took place not more than a century and a half before the discovery of America. Some Taino families reached Puerto Rico and Hispaniola, but none ever touched Jamaica, the only island of the Greater Antilles where Ciboney culture alone flourished until the conquest by the Spaniards.

At the time of the discovery of Hispaniola, as is pointed out in the old Chronicles, the Taino had pushed back the Ciboney of Haiti and had the hegemony of at least three of the five principal chieftainships—Xaragua, Marien, and Magua. Approximately fifty years before this time, some Taino of Hispaniola reached Cuba and occupied the most eastern region of the island opposite Haiti.[40]

The Taino of Hispaniola developed their own culture, endemic to that island and the most advanced and progressive of the Antilles. To that Taino culture, Rouse has applied the name Carrier.[41] Without any doubt, the Taino culture represents the climax reached by the natives of the Antilles.

With the Taino settlement in Hispaniola and Puerto Rico begins Period IV, the last one in the prehistory of the Antilles, which ends with the conquest and colonization of the islands, first Hispaniola and then, some fifteen years later, Cuba.

Tainan Culture

According to Rouse[42] the characteristics of the Tainan culture in Haiti are:

1. Settlements larger than those of the Ciboney, with a certain ordering of the houses that provided free spaces for streets and plazas or "bateys." The houses continued to be circular or polygonal in form, like those of the Ciboney. Rectangular houses, or "bohios," came into use after the settlement of the Spaniards in Hispaniola.

2. The ceramics offer characteristic differences from the Ciboney in numerous details. New forms, such as the water bottle, appeared. The ceramics comprise four types: cooking bowl, water bottle, griddle, and bead. Of stone are the petaloid celt—the same as that of the Ciboney—hammers, grinders, polishers, chisels, smoothers, sinkers, axes, small shovels, and flint daggers and knives. There are chisels, spades, pickaxes, and punches of shell; pendants and beads of bone; and also numerous pieces of flint.

3. Representation of zemis demonstrates a worship called "Zemiism" in contrast to the "Behiquism" of the Ciboney.[43] A more advanced aristocratic organization with specialization of labor perfectly established. In general, a well developed social organization, perhaps with the existence of private property.

4. Agricultural economy based upon yuca and maize, and also hunting and fishing.

The cooking pottery is well elaborated and,

[38] *Ibid.* [39] *Ibid.*

[40] Las Casas, 1909; Martir de Anglería, 1944.
[41] Rouse, 1941. [42] Rouse, 1941.
[43] Morales Patiño, 1943.

like the Ciboney, consists of open bowls, either round or shaped like a boat. The construction is very similar to that of the Ciboney, but the shape and decoration differ.[44] Bonework is rare; shell artifacts and stonework are similar to the Ciboney.

In the Tainan culture of Haiti is found a series of types which belong to Period I (Guanajatabey) and seem to have persisted until Period IV and to have been taken over by the Taino. Particular examples are the flint dagger and knife. The stone hatchet, the petaloid celt, and some objects of shell which belong to Period III also persisted during the whole of Period IV.

The Tainan Culture of Cuba

Father Las Casas[45] said, and archaeology confirms his assertion, that the Tainan invasion of Cuba took place not more than fifty years before the discovery—that is to say, at the middle of the fifteenth century—when Taino occupied the most eastern part of the island. They pushed the Ciboney out of Baracoa and, added the good father, "they took possession, willingly or by force, of that island, and of her people, making them servants but not slaves."

The most characteristic Tainan settlement in Cuba is the one named Pueblo Viejo (Old Town) in Baracoa, investigated by Harrington.[46] Some others, like Monte Cristo and Laguna Limones, are important also. Differences between the Tainan culture, found in the eastern region of Cuba, and the Ciboney, which occupied the island before, are the presence in the former of earth works (*cercados térreos*), sometimes referred to as "ball courts"; petroglyphs; and pottery more highly developed than in the rest of the island.[47] The Taino sites in Cuba have an average depth of only 25 to 50 centimeters as compared with an average depth of 150 to 200 centimeters in the larger Ciboney middens at Baracoa. Everything shows that the Taino occupation in Cuba was very recent in contrast to the long period of Ciboney settlement on the island.

CLOSE OF THE PREHISTORIC CYCLE

It was not until 1511 that the son of Columbus, who had succeeded in his rule of the Indians of Hispaniola, commissioned Diego Velázquez to conquer the island of Cuba. Velázquez and his men landed in a place near Maisí on the eastern end of Cuba and went to Baracoa, the largest Indian town in Cuba, where they established the first settlement. After consolidating his position in Baracoa, Velázquez sent two expeditions to conquer more territory. Narváez, one of the lieutenants of Velázquez, had the task of pursuing the Haitian chief Hatuey, who had previously incited the Indians of Maisí against the Spaniards. Hatuey was betrayed into his hands and was burned at the stake near Bayamo. Some months later Caguax, another Indian chief, was killed, and this broke all resistance of the natives, who were now leaderless and submitted to the Spanish rule.

The tragic fate which overtook these simple and innocent Indians as the result of the appearance among them of the Spaniards, whom they welcomed as gods, is a matter of familiar history. The destruction was so rapid that in less than twenty-five years the three groups—Guanajatabey, Ciboney, and Taino—estimated to number 250,000 persons in Cuba, were almost entirely exterminated.

[44] Rouse, 1939, 1941, 1942.
[45] Las Casas, 1909.
[46] Harrington, 1921.
[47] Rouse, 1942.

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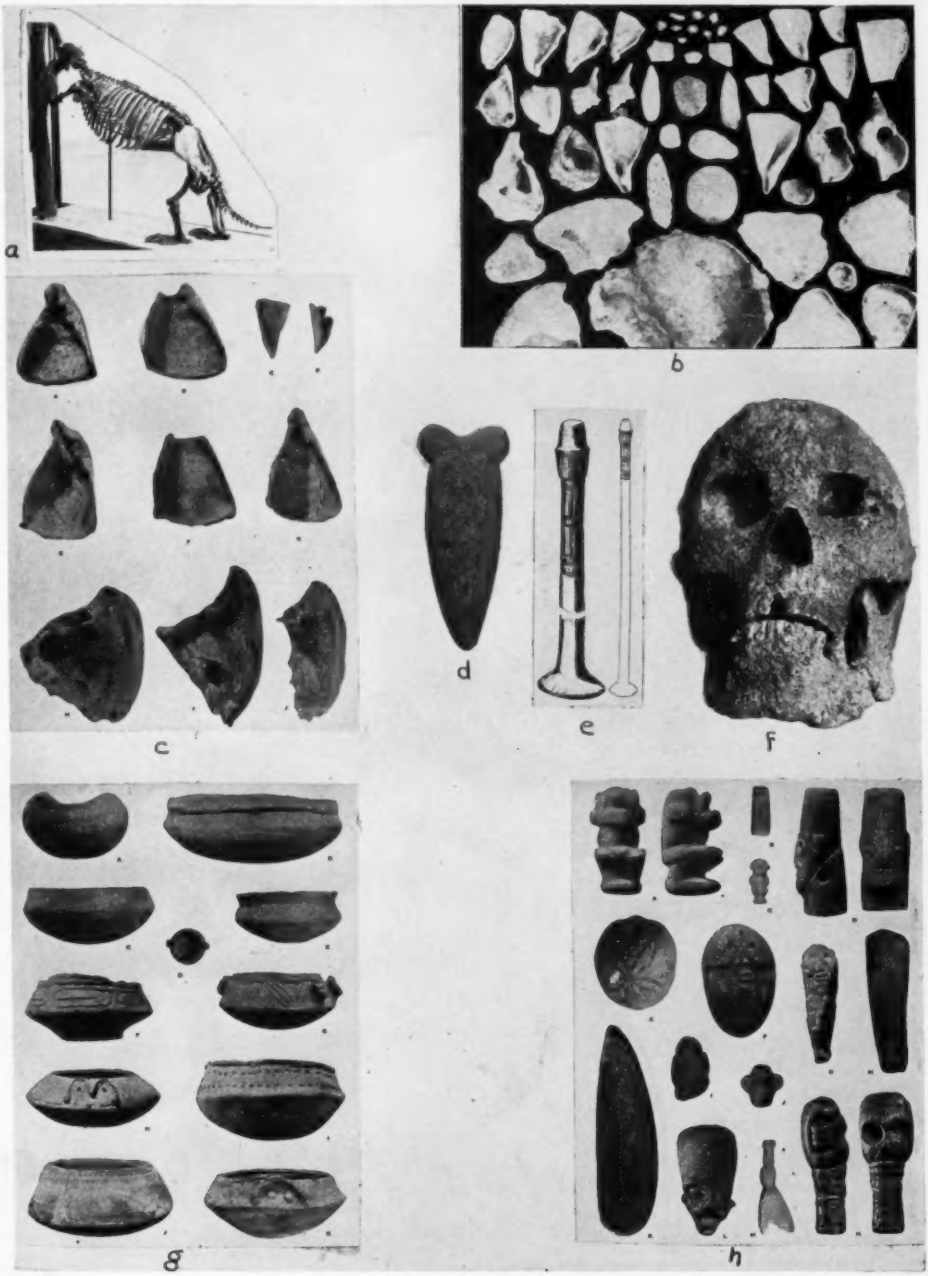
Havana
July, 1945

PLATE II

a, *Megalocnus* sp., found in archaic sites; *b*, Guanajatabey artifacts from Punta del Este, Isla de Pinos; *c*, shell implements of Cayo Redondo (A, gouge; B, sub-type, gouge; C, shell tip; D, sub-type, shell tip; E, cup; F, G, sub-type, cup; H, plate; I, subtype, plate; J, lip); *d*, ceremonial stone from Cayo Redondo; *e*, baton of wood, Guanajatabey; *f*, Guanajatabey skull; *g*, ceramics of the Ciboney culture; *h*, artifacts of the Ciboney culture. (*c*, *d*, after Osgood; *e*, after Harrington; *f*, after Cosculluela; *g*, *h*, after Rouse.)



Objects from archaeological sites in Cuba. (See facing page for identification.)

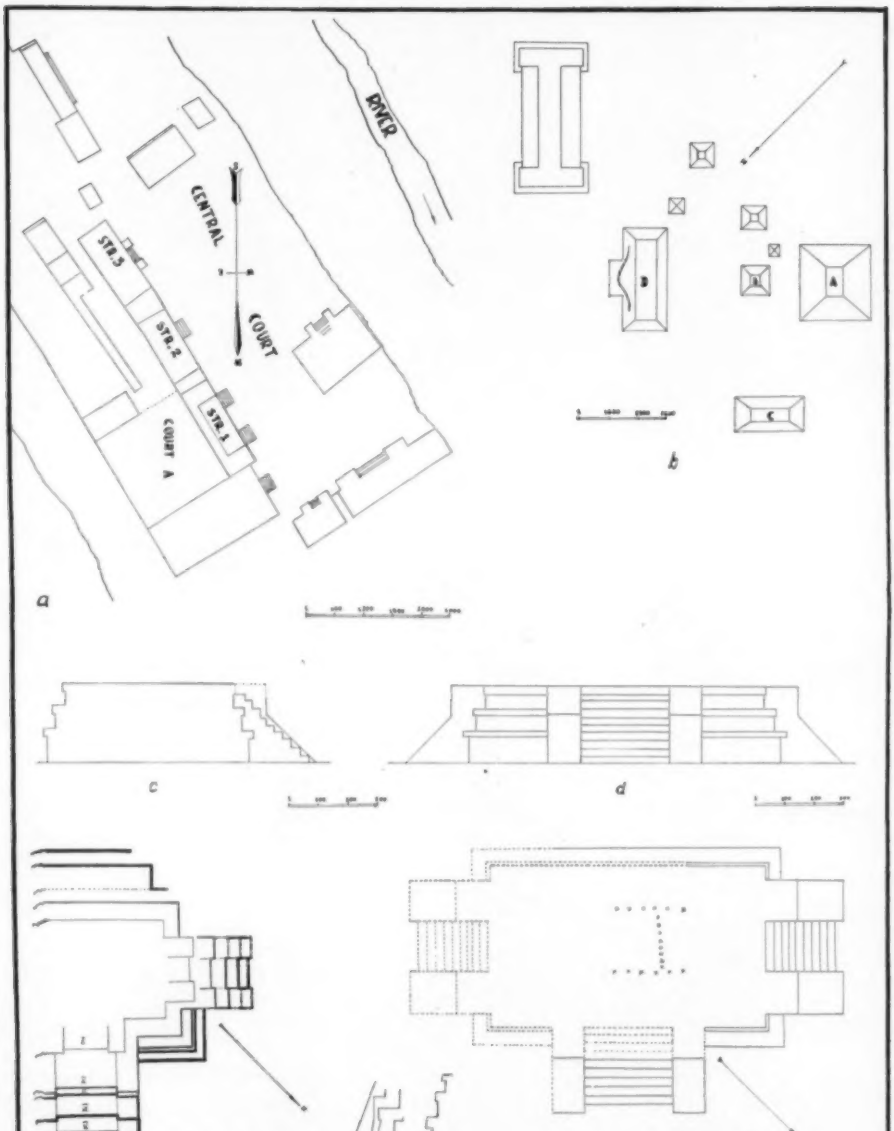


FIG. 2.—Plans and details of ruins in Chiapas. El Sumidero: *a*, general plan. San Pedro Buenavista: *b*, general plan; *c*, section of structure I of mound A; *d*, façade of structure I, mound A; *e*, plan of northern part of mound A; *f*, plan of structure I, mound A; *g*, profiles of walls of five superimposed structures contained in mound A.

ARCHAEOLOGICAL EXCAVATIONS IN CHIAPAS

HEINRICH BERLIN

WHEN the Spaniards conquered the central portion of the Mexican state of Chiapas in 1524 they found it occupied by the so called Chiapanecs, a brave tribe which the Aztecs never permanently subdued, but who on the contrary, succeeded in seriously disturbing commerce between the Aztec metropolis and their most southern provinces.

Both the origin and the history of the Chiapanecs are still unknown. There is no codex of the time prior to Cortés, nor a history written by a Chiapanec after the conquest either in Chiapanec or in Spanish. We do not even know the name of a ruler prior to the coming of the Spaniards. Therefore, in order to reconstruct the past of the Chiapanecs we have to use Spanish chronicles, comparative linguistics and the results of archaeological excavations.[1]

Linguistics points to a clear relationship of the Chiapanec language with that of the Chorotega-Mangues of Nicaragua. The linguistic relationship is fully supported by traditions recorded by early Spanish chroniclers. Dealing with the Chiapanecs, Remesal states, "Vinieron antiguamente de la provincia de Nicaragua unas gentes."[2] On the other hand, Torquemada, changing the direction, writes, "Los viejos dicen que los indios de Nicaragua y los de Nicoya (que por otro nombre se dicen Mangues) antiguamente tuvieron su habitación en el despoblado de Xoconochco."[3] The historical sources in themselves are still insufficient to indicate beyond doubt the direction of the migration, although one might think that Remesal, living so close to the Chiapanecs, would be more likely to recover the correct tradition than Torquemada, who wrote his book in the far-away city of Mexico. Nevertheless, Lehmann, after a careful study of the sources, is of the opinion that the migration was from north to south; that is to say, he holds that the Chorotega-Mangues came from the north, not that the Chiapanecs came from the south.[4] Whatever the direction, there must have been a migration in rather far-away times of Chiapanec-Chorotega people.

Under such circumstances it was thought that excavations in the region of the Chiapanecs would furnish more material to clarify the Chiapanec-Chorotega relationship. Moreover, it was thought that such excavations would be profitable, as the plateau of Chiapas lies exactly between the homes of two well-known high cultures—Maya and Zapotec. Influences of both cultures could therefore be expected, possibly in a combined form. It remained only to find a suitable site.

By means of a study of place names, Jorge A. Vivó had already established the borderline of the old Chiapanec territory.[5] Inside of it only a few archaeological sites are known. Berendt had seen one;[6] others were reported by Sapper.[7]

Later on, Marcos E. Becerra discovered new ones in a place commonly called El Sumidero, where the big Grijalva river penetrates into the mountain range which closes the central Grijalva valley to the north.[8] These ruins, considered by Becerra to be the first settlement of the Chipanecs after their migrations from Nicaragua, have been briefly described by him. The same place was also visited by E. J. Palacios, who has made brief mention of it.[9]

As Becerra's information seemed to indicate that this site would be of great interest, it was chosen as the first one at which excavations were to be made. These lasted from January 24, 1941 until February 18 of the same year.

[1] The excavations referred to in the present paper were sponsored by Dr. Rafael Pascasio Gamboa, then governor of the state of Chiapas, who always showed the liveliest interest in archaeological progress in this state, and by Dr. Alfonso Caso, head of the Instituto Nacional de Antropología e Historia. I am deeply grateful to both of them for their generous support. Thanks are given also to Bernardo Reyes, director of the local Museum at Tuxtla Gutiérrez and to Fernando Castañón of the same town for their valuable assistance. Last, but not least, I am indebted to Jorge A. Vivó, excellent friend and companion, for his untiring cooperation from the beginning of the excavations to the writing of this paper.

A complete field report in Spanish of the present paper can be consulted at the Instituto Nacional de Antropología e Historia in Mexico City.

[2] Remesal, 1932. [3] Torquemada, 1723.

[4] Lehmann, 1920. [5] Vivó, 1942.

[6] See unpublished manuscript of C. H. Berendt, "Chapanekische Studien," in the Brinton Library in Philadelphia, where it is catalogued under Br. 498 12 C.B. 457.

[7] Sapper, 1895. [8] Becerra, 1923.

[9] Palacios, 1928.

EL SUMIDERO

General Situation.—The ruins of El Sumidero can be reached from Tuxtla Gutiérrez, capital of the State of Chiapas, by taking the highway to Chiapa de Corzo as far as the bridge over the Grijalva River (which is also called Río Grande or Río de Chiapa), from where one has to take a boat right down to the famous Sumidero, a name which is used to designate the deep canyon cut by the Grijalva through the mountain range. Since it is extremely difficult, not to say impossible, to traverse this canyon, it is the common, though erroneous, belief that the river submerges and appears again on the other side.

The ruins are situated at the entrance of the canyon, on its right margin. According to the map, their position is 93°3′ west of Greenwich and 16°49′ north latitude. They stand on a sand bank composed of river deposits. The bed of the river lies actually about 25 meters below the level of the ruins. River action is at present cutting away the steep rocks on the left side and at the same time depositing sand on the right margin.

The width of the sand bank where the ruins stand is about 60 meters; then rocks rise steeply, forming the other side of the canyon. The position of the ruins therefore constitutes a perfect trap; once its door towards the south where the river enters was closed by a stronger foe, escape was prevented by the steep rocks on both sides of the river and the impenetrable Sumidero at the northern end. Furthermore, the inhabitants were exposed to stones which could be thrown down by an enemy from the height. In fact, the whole area is today completely covered with big stone blocks. It is true that their presence is not necessarily due to the action of an enemy but that quite possibly they fell down naturally. Indeed, I myself experienced such a fall of a loosened rock and observed its perfectly visible trail on the slope. Hence, the military value of the site is nil and, as there is no land for cultivation in the immediate neighborhood, it could not have served as a center for a larger population either.

The Sumidero ruins cover an area of 467 by 60 meters and consist of small mounds which are mostly placed around courts. The height of these mounds varies between 30 and 200 centimeters. I could not find any evidence that they had borne houses or temples built of stone. It is probable that these were made of wood and destroyed when the place was abandoned, but as none of the mounds was cleared, we lack proof for this supposition. Figure 2*a* shows the center of the whole ruins.

Architecture.—The main building (Pl. III, *a*) is a pyramid situated in the central court. Departing almost 45 degrees from the customary orientation, its corners rather than its sides face the cardinal points.

This pyramid has been badly damaged by treasure hunters who have taken away important parts of it, e.g. the stairway. The northeastern part is best preserved. There, a small excavation was made in order to study the foundation of the structure. This was built directly on the river sand, in which no sherds were found.

Of the staircase, which faced southeast, only the balustrades were left. The upper part was vertical and the lower part on a slant, but, as they were not completely excavated, no definite profile can be given. They were simply leaned against the walls of the pyramid, but not structurally fastened; it seems that the steps of the stairways were also not fastened, but merely rested against the wall of the pyramid. There are other, smaller staircases in other mounds, but I could never find a structural fastening.

The inclination of the walls is about 60 degrees. No mortar was found in the parts exposed to the weather, but it was found on the lower portion of the pyramid which was still covered with earth. Some stones showed a reddish color; perhaps formerly the whole pyramid was so painted.

Beneath the pyramid, on its northwestern side, close to the northern angle, was tomb 1.

In front of the pyramid, but separated from it by the central court, there is a small mound, a sort of a *momoztli*, with the same orientation as the pyramid. Its staircase must also have been on its southeastern side. A pit was dug in this mound, and tomb 2 was found.

The northeastern side of the court at first sight gives the impression of a continuous wall, but more careful study reveals three different structures (structures 1–3).

On the façade of structure 2 (Pl. III, *b*) two types of construction can be seen: an external wall, partly fallen down, and the markedly different inner one. The interior wall, type I, is built of stones practically unhewn and simply arranged, the exterior one, type II, is of stones which are cut, at least on the visible side. This

difference once established, serves to classify all the structures; there are those which have only walls of type I and those which have walls of type II. In the latter case one has to investigate to determine, in each instance, whether the wall does not hide another wall of type I, as in the cited example. Thus, in the entire section northwest of the central court, I found only structures having walls of type I, but never type II. On the other hand, in the southeastern section both types are to be found. According to my observations, there exist walls of type II built over walls of type I; only in one case does a structure consisting only of type II masonry seem to be built on to another structure with walls of type I. In the pyramid itself only type II was found.

Type II constitutes an advance as compared to type I. The fact that in several cases it overlies type I suggests that it is more recent, although of course they could also have been contemporaneous. If type I is really the older, this would mean that the whole northwestern section is older than the central and southeastern sections, or, in other words, that the process of building advanced from northwest to southeast. We have seen that the pyramid contains only walls of type II. Since I could find no substructure, it would seem to be younger than the northwestern section. That means it was not always the main center of the site, in spite of the fact that today it is comparatively the most important structure.

One can, of course, believe that type I only constitutes the structural nucleus for type II. If this were true, the above conclusions would stand nullified. Nevertheless, our theory is strengthened by the lack of type II in the entire northwestern section. There I could not find even fragments of it, and it is hardly to be believed that it has been completely removed in modern times—it frequently happens that farmers take stones from archaeological buildings for use in their houses or for fences—just from the place nearest to the entrance of the Sumidero. Neither is it probable that the structures there were not finished, as perfect stucco floors were found. Furthermore the pyramid, precisely where one would expect a stone nucleus, does not have one.

Of the three structures forming the northeastern side of the central court, structure 1, opposite the pyramid, has the best developed ground plan. Two stairways with four (?) steps lead to the summit, the platform of which has small elevations on three sides. When a pit was dug on this platform, several stucco layers of different thicknesses had to be pierced. The marginal elevations, too, have various stucco layers. A structure with a similar ground plan and also with walls of type II was found in the southeastern section.

Structures 2 and 3 are of less interest and they are still lower.

Towards the northwest, the court is closed by some low structures with walls of type I and towards the southwest the slope falls sharply to the river. No investigations were made to ascertain whether the slope was artificially corrected, as one might suppose.

Tombs.—As already stated above, two tombs were found. Since, during the excavations, means to take out the bones in a manner which might guarantee their proper preservation were not at my disposal, and as it was then hoped to continue the excavation during another field season with the assistance of a physical anthropologist, I thought it wiser to leave the bones where they were and covered them after removing the pottery.

Tomb 1 was found beneath the pyramid, on the northwestern side, the skeleton lying in foetal position. Distribution of this position has been studied by Javier Romero,[10] who gives a fairly complete list of other sites in Mexico where it is found. The southernmost place which he mentions is Monte Alban in Oaxaca. The new discovery at El Sumidero extends the area considerably to the southeast.

In front of the skeleton was found the "offering," consisting of a tripod bowl near the right foot and a jar near the left (Pl. IV, *a*, *c*). The tripod bowl is of a light brown clay, well fired, without painting. Its height is 11 cm.; its diameter at the top, 15.5 cm.; and its maximum diameter, 17 cm. Although the three feet are hollow, with a round hole on the outside, they have no pellet to give the effect of a rattle. The jar is 17.5 cm. high, the diameter at the top is 10 cm., and the maximum width is 17.5 cm. It is a plumbate jar of greenish color. On the neck is a meander design, drawn with little skill. Nothing was found in either vessel except the sand which covered the whole grave.

Tomb 2 was found in the *momoztli*. The skeleton was in the supine position with the

[10] Romero, 1937.

head to the southwest. Besides the skull and the shin and thigh bones, few other bones were preserved. Of the teeth two upper and three lower incisors were found. Neither mutilation of teeth nor cranial deformation was noted, although these might be recognized by a physical anthropologist.

The skeleton lay in the river sand which constitutes the base of the whole area of the ruins. Above the skeleton, and on the actual level of the court, there were large stone blocks, and then a regular filling of earth with a few stones reached as far as the platform of the mound. No stucco floors were found. This arrangement seems to suggest that first the tomb was excavated, then the large stone blocks were put on top of it, and finally the mound was built, so that the tomb is older than the overlying mound, not vice versa.

Here, too, the "offering" consisted of two vessels (Pl. IV, *b*, *e*). These had been placed upon the body, but later the jar had slipped down between the two thigh bones. This jar is an imitation of a squash. It is of plumbate ware, vermillion with greenish spots. Its height is 12 cm.; its diameter at the top, 6 cm.; its maximum width, 11 cm. The other piece is a tripod bowl with slit legs of the cascabel type containing a pellet. The clay is light brown, well fired, and without painting. Its height is 6.5 cm., its diameter at the top, 14 cm.

Although the two "offerings" show certain differences, essentially they are alike in so far as both consist of a tripod bowl with round legs accompanied by a plumbate jar. Both were found in structures belonging to our type II.

A third "offering" (Pl. IV, *d*, *g*), not with a skeleton, was found in a type I mound in the northwestern section. There we dug a pit, and 40 cm. below the actual surface we found a stucco floor, which we pierced. Immediately below this floor was found the "offering," consisting again of two vessels. One of these was a jar (height, 13.5 cm.; top diameter, 6.5 cm.; maximum width, 12 cm.) of a reddish, very sandy clay. It has three stumps indicating the places of the handles. The other piece (height, 7 cm.; maximum length, 14 cm.) broken and full of snail shells, has a reddish-brown slip. It is an effigy vessel with the head of a tiger or coyote and with two forelegs. It is similar in execution to various pieces of ceramics the provenance of which is unknown in the local museum at Tuxtla Gutiérrez. These have zoömorphic decorations: just the head and the two forelegs are modelled but hindlegs or tail are not even indicated. The bowl just referred to was the only effigy vessel found during the excavations.

Ceramics.—Pits of various sizes were dug in different places throughout the area. Digging was always suspended as soon as the fine white deposit of river sand free of sherds was encountered. These pits were dug in structures 1 and 2, one in the northern area where the third "offering" was found, one in the southern section, one in court A, and, finally, one in the *momoztli*.

Of course I should also have liked to make an excavation in the pyramid itself, but there treasure hunters had already dug, opening a big funnel-shaped crater, thus making it unprofitable to dig another pit.[11]

The results obtained by tests of all pits, are, in broad outlines, the same. The supposition of two different periods which seems indicated by the architectural differences is not backed up by the study of the ceramics. Nor can any clear-cut sequence of ceramics be observed.

The pottery is always very coarse and of little artistic development. A total of 3085 sherds was recovered, all so small that it was not possible to reconstruct entire pieces. There were few rim sherds, almost all of which are illustrated in Figure 3. No legs of vessels apart from those of the above-mentioned tripod bowls were found during the excavations. Only on the surface were some solid conical legs found. The absence of handles is likewise noteworthy—all those found can be seen in Figure 3. Painted ware is practically nonexistent at El Sumidero.

The main classes of wares are listed below. The color indicated refers to the clay and is in the great majority of cases identical with that of the surface of the sherd. Shapes are illustrated in Figure 3.

Type 1 (59 per cent of all pottery): coarse, with thick walls, light brown or cream, rims curving outward, used in larger vessels. No slip and no polish.

[11] A description of at least some objects found in this pyramid is given by Becerra, 1932*a*: "... La señora me propuso en venta y yo le compré algunas piezas que su esposo había sacado de las ruinas, probablemente del montículo piramidal. Es evidente que Ralda, cuando nosotros hubimos dejado el lugar se dedicó a explorar solo y halló lo que la señora me vendió y que fué: un apastillo u olla de barro, medianamente fino, con una tapa a modo de comal, dos pendientes de jade con sus correspondientes perforaciones, una pieza a manera de tau o cruz sin cabeza ni agujero alguno por donde colgarlo de marmol no bruñido impregnado, en su superficie granujienta, de una pintura roja."

Type 1a (6 per cent): like type 1, but overfired, thus of black appearance.

Type 2 (12 per cent): like type 1, but polished and sometimes with a reddish slip. Very occasionally with simple designs.

Type 3 (11 per cent): like type 1, but with thinner walls.

Type 3a (3 per cent): like type 3, but polished or with slip.

The percentages of types 3 and 3a generally increase in the upper strata.

Types 1 to 3a, constituting 91 per cent of all ceramics, can be described as "brown to cream clay, coarse and sandy."

Type 4 (3.6 per cent): ochre clay, well fired, fine texture, sometimes with no temper visible to the naked eye, with a brown slip. Sometimes drawings of simple lines. Used in small bowls.

Type 4a (2.7 per cent): brown ochre clay, well fired, without polish, medium walls.

Type 4b (0.5 per cent): like type 4a, polished, with thicker walls and used in larger vessels.

Types 4 occur, proportionally, more frequently in the lower strata.

Type 5 (1.1 per cent): light brown clay, sandy, like type 1, but with raised decorations sometimes made with the fingers only.

In addition to the above, small quantities of two other types were found:

Gray clay (0.9 per cent): The few sherds were found in different strata of various pits. There were nine sherds of gray clay with a cream slip. Since the percentage of gray ware is so low, one feels inclined to consider it imported.

Dark brown clay (0.2 per cent): very sandy, thick walls with a white slip, painted brown lines.

No figurines were found.

Other objects: two bark beaters found lying on the surface, one ear plug, two manos, one arrow point of quartz.

The results of the excavations can be summed up tentatively as follows: El Sumidero was a site of limited importance. It was in existence during the plumbate epoch during which at least part of the mounds were built—provided that we are correct in assuming the *momoztli* and the plumbate offering it contained to be contemporaneous. Another part probably was built still earlier, if we are right in our distinction between walls with cut and uncut stones. We believe the mounds with cut stone walls are more recent, coinciding with plumbate ware. The excavations did not yield sufficient material to prove a continuance of occupation in later times. Moreover, we lack knowledge of the cultural relics in the neighboring regions dating from the time immediately before the Conquest. Considering the references given by the Spanish historians, it is likely that the site was occupied by the Chiapanecs, although it is well to bear in mind that it lies precisely on the border line between the Zoques and the Chiapanecs. This means that the former might have been the builders of the Sumidero mounds, especially if one considers Herrera's[12] statement that the Chiapanecs, on arrival, found the region occupied by the Zoques.[13]

CHIAPA DE CORZO

Mounds to the north of the modern town of Chiapa de Corzo were first recorded by Berendt[14] and later by Sapper.[15] However, no excavation was carried on until Mr. Jorge A. Vivó dug the first pits to collect ceramics in 1941. He took advantage of the fact that a cut which was made through one mound in order to build a highway to Ixtapa exposed different floors. The collected sherds were turned over to me for comparison with those found at El Sumidero. Sherds are illustrated in Figure 3.

The first group, (type 6, 27.3 per cent) is of a cream to brown clay, unpolished, with sand temper and decorated with one or two incised lines. No outward sloping rims were found.

The second group (type 7, 60.4 per cent) is similar to the foregoing one, although its texture is somewhat finer. It is either polished or has a slip, usually red. It was generally used in smaller pieces such as *cajetes* or deep plates, which are to be found frequently in the whole Chiapanec area; the museum at Tuxtla Gutiérrez has some complete pieces on exhibition.

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[12] Herrera, 1601.

[13] For the determination of the age of the Sumidero ruins we did not use the analysis of the pottery, as this is for the moment too undistinctive. I should, nevertheless, like to stress the relationship between our type 5 and similar sherds found by Dutton and Hobbs at Tajumulco (1943, Fig. 82, *k, l*), a place also occupied during the plumbate era. Acosta (1942–1944, Fig. 27) considers very similar sherds at Tula as typical for the Tula-Mazapa epoch which coincides with plumbate. This strengthens our belief that the Sumidero site also belongs to this epoch. I should also like to mention that this type 5 was always found in deposits more than 50 centimeters below the surface. But, as the total of type 5 sherds was only 35, this fact may well be coincidental.

I had a chance to get a glimpse of the material M. W. Stirling excavated at Ocozocuautla, a site in the Zoque region not very far from El Sumidero and had the impression that between these ceramics there must be a relationship, but this can only be definitely established when Mr. Stirling publishes his findings.

[14] See note 6. [15] Sapper, 1895.

Type 7 seems to have diminished in the upper strata, where more of type 6 is represented.

The third group (type 8, 6.7 per cent) is nothing but a variation of type 7, having a completely black slip. Here, too, the vessels were of small dimensions. Sometimes incisions served as decoration.

The remaining groups (5.6 per cent) are of no importance to the general picture.

It certainly would not be wise to pretend to form a definite judgement of a place where only a few stratigraphic pits produced about 1500 sherds. The complete absence of painted ware, however, is probably not accidental. From many points of view the material is similar to that found at El Sumidero, although that from Chiapa de Corzo has a finer texture and a much higher percentage of sherds with slip.

On the other hand, types 7 and 8 have the most ancient aspect and remind one of Ticoman in the Valley of Mexico. This means that at least the lower strata of the mounds at Chiapa de Corzo might for the moment be considered to be associated with the upper archaic of the Valley of Mexico.

The fact that the mounds at Chiapa de Corzo yielded such old ceramics without any trace of definitely more recent material being present in the upper layers makes it plausible that they were already abandoned when the Spaniards arrived and that they had nothing to do with the then main settlement of the Chiapanecs. It confirms the hypothesis of Marcos E. Becerra[16] that this main settlement was situated on the *left* side of the Grijalva River, a hypothesis based on the description of its conquest given by Bernal Díaz del Castillo, although the exact geographical situation of the settlement has not yet been established.

Since Díaz del Castillo called it a real town with more than 4000 inhabitants, it could have been situated only between the confluence of the Santo Domingo and Grijalva and the present bridge "Belisario Dominguez." From Remesal,[17] we learn that in 1528 the population was reduced to 2000 inhabitants whom the conqueror, Diego de Mazarriegos, forced to settle on the spot where Chiapa de Corzo stands to-day. Since their original town was destroyed after the rising against the Spaniards, it is easy to believe that the Chiapanecs were willing to establish themselves on the more fertile right side of the river, now no longer needed as defense against their mortal foes, the Aztec garrison of Zinacantan, likewise conquered by the Spaniards.

While travelling through the Chiapanec region, Mr. Vivó recovered some sherds, all of which are coarse and sandy. They were found lying on the surface. With the exception of one fine polychrome tripod bowl, the museum at Tuxtla Gutiérrez does not possess any fine ceramics of Chiapanec origin. No influence of the nearby Mayas or Zapotecs has ever been seen.

The above-mentioned tripod bowl, from Chiapa de Corzo, has conical legs and almost vertical walls. It is decorated both inside and out. The bottom has a kind of stamped decoration. At first sight it seems to belong to Lothrop's Nicoya polychrome ware, but after closer examination it appears that it bears resemblance only to the Managua ware, which, of all Chorotega-Mangue wares, is the one that by its use and forms resembles the Maya and Aztec wares. Since it is unique in the Chiapanec region, it would appear to have been imported and can by no means be considered as a typical example of Chiapanec pottery.

It seems that the Chiapanecs were never good potters, considering middle American standards, and that finely decorated pieces occurred only sporadically. Their linguistic relatives, the Chorotega-Mangues, on the other hand, were producers of excellently painted ceramics influenced by elements outside of Mesoamerica. Hence it seems that the separation of Chiapanecs and Chorotega-Mangues must have taken place a very long time ago, enabling the Chorotega-Mangues to develop their ceramics independently. Lehmann,[18] relying for his analysis on the historic sources, thinks that the separation occurred before the sixth century. Our present archaeological knowledge is not contradictory to this supposition and rather tends to confirm it, although it can say nothing as to the direction of the migration. It is similar to the well-known case of Huastecs and Mayas, where the linguistic relationship exists to the present day, whilst the ceramics, apart from the oldest substratum, which is similar in both areas, never showed a notable or specific likeness.

SAN PEDRO BUENAVISTA

Whilst the excavations at El Sumidero were proceeding, I visited the site of San Pedro Buenavista, guided by a real "treasure hunter"

[16] Becerra, 1932*a*.

[17] Remesal, 1932.

[18] Lehmann, 1920.

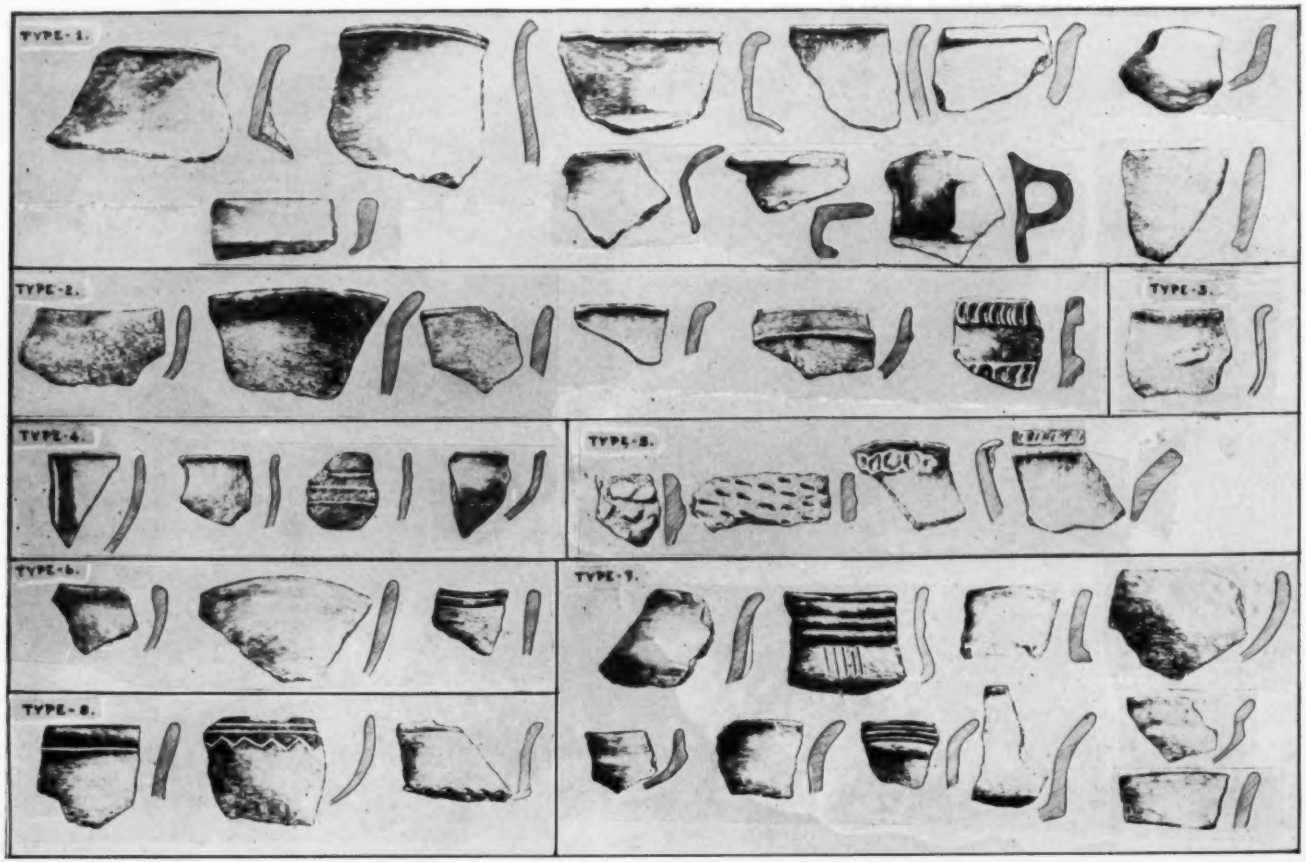

FIG. 3.—Pottery from Chiapas. Types 1–5, El Sumidero; types 6–8, Chiapa de Corzo.

Archaeological excavations in Chiapas, Mexico. El Sumidero: *a*, main pyramid; *b*, view of structure 2, showing walls of types I and II. San Pedro Buenavista: *c*, mound A, west corner; *d*, main stair of structure I; *e*, mound A, north corner.

of Tuxtla Gutiérrez who had made excavations there on his own account. And it was owing to his "preliminary work" that in one of the mounds a series of superimposed structures was visible. As these seemed to be of importance, the site was chosen for another brief field season, from December 22, 1941 to January 17, 1942.

The ruins are situated in the neighborhood of rancho San Pedro Buenavista, from which their name was taken. This, in turn, is about four kilometers distant from the village of Suchiapa and lies in the valley connecting Suchiapa and Teran. It is at San Pedro Buenavista that the Santo Domingo River, coming from the southwest, enters the valley and makes a sharp turn towards the southeast. The ruins lie within this bend, being thus protected by the river on the northwest and northeast. But this protection is more apparent than real, since during the dry season the river can be easily forded at every point. On the other hand, the river provides a perennial water supply, always a considerable attraction in this calcareous region, which is poor in water.

The orientation of the mounds is essentially the same as that of the one at El Sumidero, 45 degrees from the cardinal points. This orientation seems to be typical of Chiapanec sites, as it is found also in the ruins of San Vicente and Las Mercedes, according to the plans drawn by Sapper.[19] Sapper mentions this orientation also in connection with the ruins of Bolonchac (Departamento La Libertad), situated in a territory which linguistically is Maya, but is very close to the Chiapanecs.

To the south, Lothrop has found it in Zacualpa,[20] and, to the north, Caso has demonstrated it for Tilantongo and Yucuñudahui in Oaxaca.[21] Since, during the so-called old Empire of the Mayas, buildings were generally oriented with their sides facing the cardinal points, the orientation of 45 degrees might be considered as a first evidence of comparatively recent origin. On the other hand, of course, this orientation sometimes occurs much earlier, as, e.g., in the case of mound J of the second epoch at Monte Alban. We have seen that the ruins of El Sumidero probably are not much older than plumbate. As will be demonstrated, San Pedro Buenavista is not very old either.

In the eastern corner of the ruins (Fig. 2, *b*) there is a ball court which the inhabitants of the rancho commonly call "la iglesia" (the church). Although it covers a fair surface, the surrounding walls are extremely low. They are never higher than 1.60 meters. Were it not for the unmistakable shape of its ground plan, a double T, one really would hesitate to identify it as a ball court. A transverse trench was cut in the center of the court, but no floor was found. The lateral walls are very badly constructed of big stones, loosely arranged into rows with slabs leaning against them. The surface of the walls slopes at approximately 33 degrees. Neither mortar nor cut stones were found. The extremely sandy reddish earth of the region was used to fit the stones together.

In Figure 2 *b*, the structures of main importance are indicated with capital letters, A, B, C and D. At the present stage of the excavations, structure A appears to be the most important. Next in importance is mound D, about 4.50 meters high and apparently untouched. Its ground plan makes it seem likely that a stairway of greater importance will be found on its northeastern side. As funds sufficient to make a complete excavation there were not at my disposal, I abstained, very unwillingly, from starting one, postponing it to the future. Mound C was not excavated either.

More attention was paid to mounds A and B, and excavations were made in both of them.

As the "preliminary work" of the treasure hunter had laid bare the northwestern part of mound A, I determined to study that portion more thoroughly. My predecessor apparently always removed the rubbish until he reached a solid wall and, as the walls of the five superimposed struc.ures in the mound varied in hardness in different places, he sometimes stopped at the fourth building, at other times at the second one, etc.

With only a limited number of workmen I could not uncover the whole mound but had to limit my work to trenches and supplementary excavations which permitted me to make the plans and an attempted reconstruction on paper. In the plans I have indicated with solid lines that which is really still existent and with dotted ones that which has either completely disappeared, especially on the northern side of the mound, or which still remains undiscovered, but can be supposed to exist by reason of symmetry. The latter condition is true especially of the southern side.

Thus five superimposed structures could be proved, all following the same scheme. The

[19] *Op. cit.* [20] Lothrop, 1936. [21] Caso, 1938.

main staircase faces northeast, and there are lateral ones on the northwest and southeast, but none exists on the southwestern side.

Only the innermost structure I is well preserved and its platform is the platform of the mound as it stands to-day, which means that whatever of the structure was higher than this level has now completely disappeared.

Figure 2, *c*, *d*, and *f*, shows a transverse cut, the façade and the ground plan of structure I. This consisted of the stairways mentioned and of three perpendicular walls, all of which had an upper cornice. Since the uppermost wall has been damaged at the place where the cornice should be, we cannot be certain that there was a cornice, although it is plausible, since the cornices on the two lower walls are still visible. Neither do we know exactly how the vertical part of the balustrade connected with the platform.

In the second structure, the staircases were enlarged and the balustrades of the lateral staircases were made somewhat broader, while those of the main one were made considerably broader (Figure 2, *e*). It is impossible to determine of how many walls this structure—and of course all the later ones—was composed. But its walls were still perpendicular. (See Figure 2, *g*, which shows the sequence of the walls in the five structures.)

In the third structure, sloping walls were used in place of perpendicular ones, although here the inclination is not yet pronounced. Stairways and their balustrades were built exactly over those of structure II, but they were not made broader. For the western angle, the data to connect the posterior wall of structure III with the corresponding lateral staircase have not been found, but from what is still existent I suppose it to have been symmetrical with the northern angle.

If the connection of the posterior wall and the northwestern staircase was uncertain in structure III, it is still more uncertain in structure IV, but it can be stated that symmetry has been abandoned, as indicated by the asymmetrical corner in the western angle. Of the walls, only the first one exists, and it is much more inclined (Plate III, *c*).

Although only a very few are preserved, there are, in the fifth structure, elements which are the same as those in the fourth, with the exception of the mentioned asymmetrical corner. Failure to find anything corresponding to this may be due to its destruction.

In the center of the platform of structure I, a pit was dug to a depth of 3.55 meters. Although no ceramics were found, an alabaster vase was uncovered at the level of the ground outside (Pl. IV, *f*). From the platform to the exterior ground level the filling consists of light brown sand lying over a stratum of 5 cm. of black earth. Then comes the natural ground, a very stony sand.

Structure I probably contained a tomb, as inside it there were two parallel walls, running in the direction typical of the site, i.e. from northwest to southeast. These walls, 1.30 meters high, are built at ground level. At this level they are connected by a row of large round river stones, of which the two walls are also made. The other structures were built up with slabs. In Figure 2, *f*, these walls are indicated by lines of crosses; they extend beyond the limits of the pit in both directions, but I did not try to locate the ends, for to do so would have necessitated the destruction of the fragments of the stucco platform which are the only indicators of the height of structure I. This stucco floor lies directly on the sand nucleus, a form of construction which was used also in other mounds.

The nucleus was of the same consistency in the pit and underneath the untouched stucco of the platform. Only at the upper end of the pit were some fragments of the top floor found. To account for their presence two explanations suggest themselves: (1) that the treasure hunters, who dug in from the southwest and destroyed a considerable portion of the back of the mound, moved the soil in the upper part of what later became my pit and that in the time between the two excavations the loosened sand hardened so that it could not be distinguished from the untouched parts, or (2) that in pre-Spanish times an opening was made.

The alabaster vase (Pl. IV, *f*) was covered by a flat round stone when I found it. It has three tubular legs and the head of a tiger or coyote, placed asymmetrically. It is 10 cm. long and 9 cm. wide at the top; its total height is 16 cm.; its maximum length, including the head of the animal, 17 cm. It was placed so that the head faced northeast, which means that its axis was parallel to the axis of the main stairway. It was completely empty, except for a prismatic, triangular, small, green, stone bead.

Alabaster vases are comparatively common in this region. I have been informed that two other pieces had already been unearthed from

the same mounds. Marcos E. Becerra mentions another one found in the neighboring Teran.[22]

I am of the opinion that these alabaster goods are not imported from very far, but that they might come from the quarry of nearby Zinacantan, a Maya-Tzotzil place. This quarry is mentioned by Francisco Ximenez,[23] quoting Fray Tomás de la Torre, Dominican friar who was the historian of the first group of missionaries brought from Spain by Bishop Las Casas in 1544. Since the quarry was known to the Indians in the first half of the sixteenth century, one may suspect that it was exploited already in the pre-Spanish times, particularly as the same Fray Tomás de la Torre tells us that the Zinacantan people were always at war with the Chiapanecs because of the salt mines. Hence it is even possible that the alabaster quarry of Zinacantan was once owned by the Chiapanecs. Future crystallographic analysis of our alabaster vase and the alabaster of Zinacantan will throw more light on this question.

One final piece of evidence was provided by the owner of the land where the ruins stand, Mr. Ponciano Ruíz, who died in December, 1942; he told me that he, too, had made excavations some thirty years ago in mound A. He said that on the top there was a "niche," something like a wall several meters high and facing northeast. The statement of Mr. Ruíz is to a certain extent confirmed by the subsequent finding of the main stairway, as of course one would suppose that a building on top of the mound would, like the stairway, face northeast.

While mound A was being explored, mound B was cleaned of bushes. It proved to consist of two superimposed structures, its stairway facing southwest, just opposite to the stairway of mound A. A pit dug in the center of mound B yielded only minor details as to the interior construction of the mound. Practically no pottery was found in the whole area.

As we cannot use ceramics for establishing the age of the San Pedro Buenavista ruins, we have to use the alabaster vase which we found in the center of mound A and all architectonic data recovered during excavation. These data are: a ball court in the shape of a double T; the custom of superimposing structures; a pyramid with stairways on three sides; balustrades composed of vertical and inclined walls—even the oldest structure has a balustrade of this form—vertical walls, in the older structures, with a cornice at the top and, in the more recent ones, inclined walls.

Vertical walls in pyramidal structures can be traced back to very old Maya times; although it is true that they are not very frequent, they occur at Palenque. They are more frequent in later structures of Yucatan, as, e.g., in the Monjas at Chichen Itza and in Temple 5 at Tulum. Northwest of the Chiapanec area they have been found at Monte Alban in mound J, a rather old one. Towards the south they occur frequently among the Mayas of the Guatemala highlands, as at Rabinal, Zaculeu, Santa Cruz del Quiché, and Comitancillo. These latter sites are also related in other respects to the ruins of San Pedro Buenavista, since they have the following details in common with structure I of mound A: Rabinal has a balustrade with vertical-inclined profile and three walls; Santa Cruz del Quiché has walls with upper cornices and three staircases; Zaculeu has balustrade with vertical-inclined profile and vertical walls; and Comitancillo, three walls and three staircases.

Thompson, in "A Trial Survey of the Southern Maya Area," established four periods for the southern Maya region: the formative period (prior to A.D. 300), the initial series period (325–900), the Mexican period (987–1204), and the Mexican absorption period (1204–1540). In his discussion of the Mexican period, Thompson includes the sites of Zaculeu, Rabinal, and Santa Cruz del Quiché, having a few lines before made the statement that "In the field of architecture there are a number of structures which can be assigned to the Mexican or Mexican absorption period." Furthermore, he states that these three places were Maya strongholds when the Spaniards arrived. Since no statement about the existence of superimposed structures in these sites has been published, we must believe that the buildings mentioned above served as centers for religious service when the Spaniards came. As it seems rather improbable that the inhabitants of these sites would not have changed or modified them during the 300 or more years since the end of the Mexican period, I should prefer to classify them as belonging to Thompson's last period until further scientific excavation leads to the acceptance of another interpretation.

Since the architectural features of San Pedro Buenavista are so closely related to those mentioned above, it is plausible to say that they are of more or less the same age. Fortunately, we can check this age by comparisons with ruins

[22] Becerra, 1932*b*. [23] Ximenez, 1929–1931.

situated further north.

Apart from the dimensions, there exists a notable similarity between our modest mound A and the great pyramid of Tenayuca, situated at the entrance to Mexico City. In Tenayuca, too, we find superimposed structures (7 or 8), vertical walls at the beginning (periods I and II) and inclined ones towards the end (periods III to VII), and balustrades with vertical-inclined profile beginning at least in period V.

The age of the pyramid of Tenayuca has been fixed as approximately between 1150 and 1450, i.e., starting just after the decline of the culture of Tula, Hidalgo, now justifiedly called the Toltec culture. Because of the aforesaid similarities I think that San Pedro Buenavista can be considered contemporaneous with Tenayuca, a correlation which is in perfect harmony with the ages already established.

Moreover, drawings of balustrades with vertical-inclined profiles are often found in Mixtec, Aztec, and even post-Conquest codices, thus confirming their frequent use at the time of the arrival of the Spaniards.

The fact that an alabaster vase was found in the center of mound A does not invalidate the above analysis. Although their first appearance is in late Teotihuacan times, alabaster vessels were made right up to the Conquest.

CONCLUSIONS

In summary, we can venture to say that:

1. The ruins at El Sumidero correspond to the plumbate period; the possibility exists, however, that they started even a little earlier.

2. The mounds on the right side of the Grijalva River at Chiapa de Corzo are older than those of El Sumidero.

3. Since plumbate can be related to the Tula era and since, on the other hand, the site of San Pedro Buenavista is later than Tula, by virtue of its architecture, it is also later than El Sumidero and may have flourished between 1150 and 1524. The relative "youth" of this site is perhaps confirmed by its closeness to Suchiapa, a name meaning "the new Chiapa."

All the material found in the three places studied adheres to the general pattern of Mesoamerican cultures. Nothing was excavated which showed any special relationship with Nicaraguan cultures. The tripod bowl found at Chiapa de Corzo, which might correspond to polychrome Managua, rather seems to be an isolated piece of importation.

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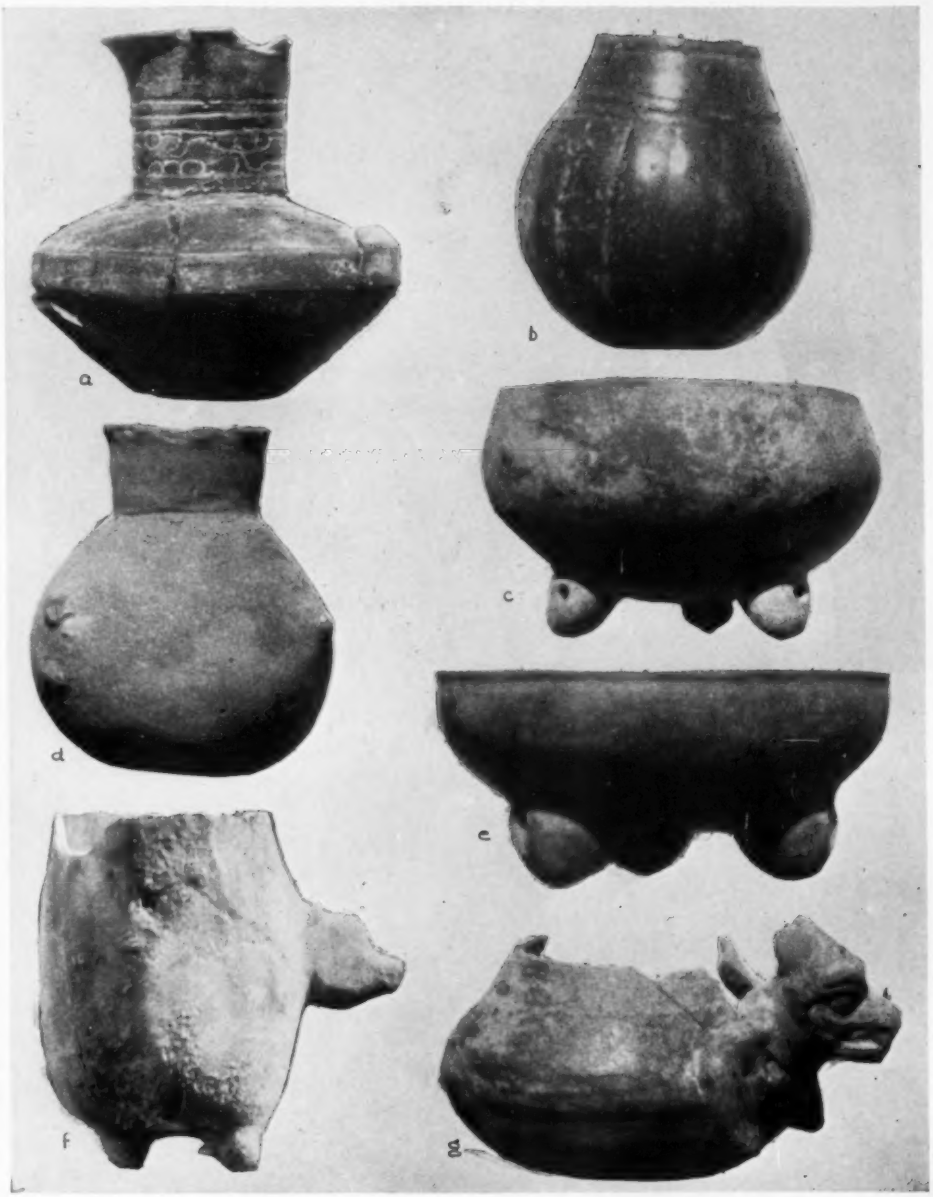

Vessels from sites in Chiapas, Mexico. El Sumidero: *a*, plumbate jar, tomb 1; *b*, plumbate jar, tomb 2; *c*, tripod bowl tomb 1; *d*, jar found in northwestern section; *e*, tripod bowl, tomb 2; *g*, effigy bowl found in northwestern section. San Pedro Buenavista: *f*, alabaster jar found in mound A.

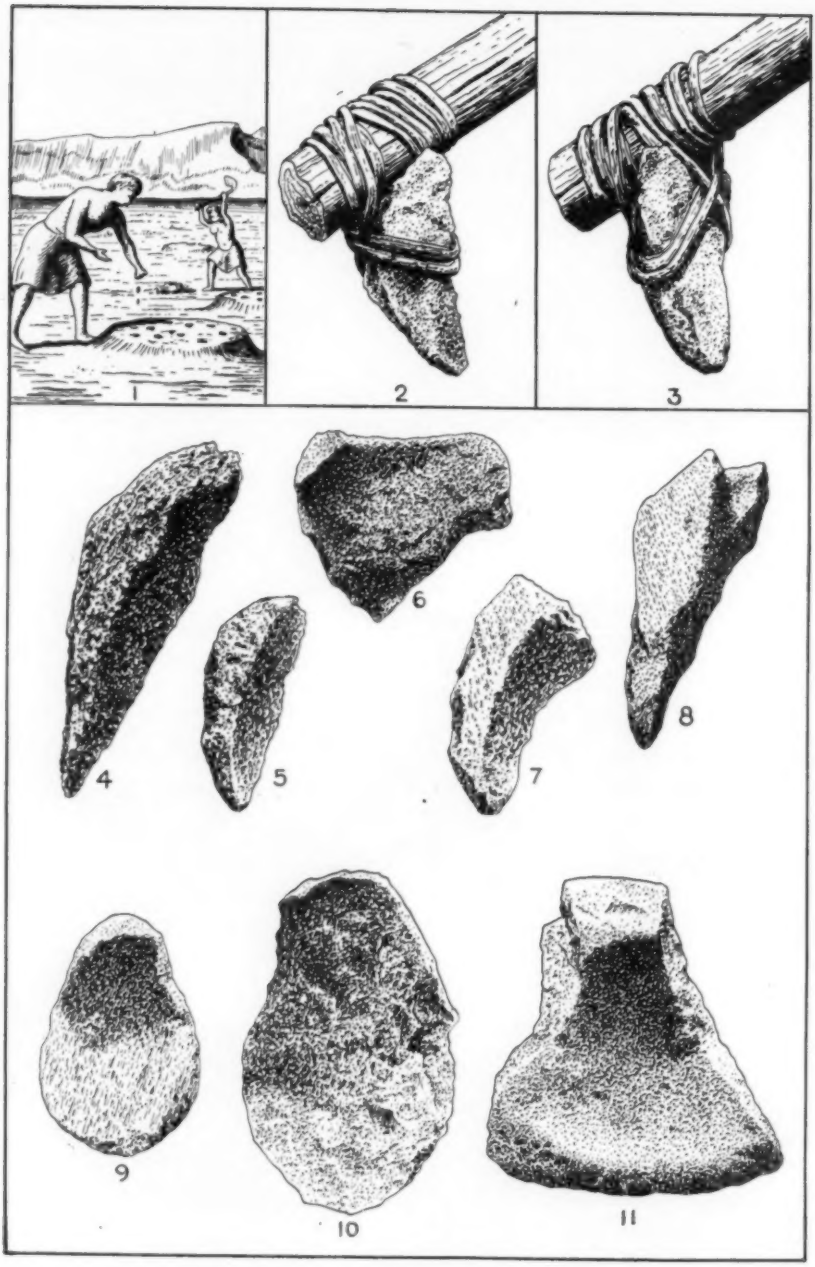

FIG. 4.—Planting hoes. *1*, agricultural practices of the Hurons (from Lafitau, 1724. Reproduced by permission of Department of Mines and Resources, Ottawa, Canada); *2–3*, probable hafting for triangular hoe; *4–8*, hoes of triangular type from Connecticut Valley; *9–11*, hoes of tanged type from Connecticut Valley. (Slightly less than one-half natural size.)

THE HOE COMPLEX OF THE CONNECTICUT VALLEY

WILLIAM S. FOWLER

THE aboriginal agriculturists discussed here lived in a somewhat secluded environment, the Connecticut River Valley from Bellows Falls to the Connecticut line. In this long-occupied territory, surrounded by high ridges of volcanic origin, heavily wooded, and watered by innumerable spring-fed streams, the cultural development of the inhabitants was apparently of a homogeneous nature. As late as 1636, when the English began to establish plantations throughout the valley, the natives were found united in a well-defined River Confederacy, with frequent intercourse maintained through river travel. While trade routes probably connected this section with many other parts of the country, cultural contacts had apparently persisted among the river tribes in spite of occasional raids of warlike groups from other regions. As a result, stone artifacts from the hundreds of habitation sites in the valley have similar traits and suggest close industrial association.

As the hoe has been connected with agriculture in many parts of the world, it would not be at all strange if it were found among the Connecticut River peoples; but the determination of which of several possible stone artifacts were hoes is difficult. During research of the last several years, the writer has made certain observations which seem to have an important bearing on this question. In the hope that they may stimulate new thought on the subject, these observations will be reviewed in this paper.

Eye-witness reports from earliest historical days confirm the belief that agriculture in New England was concerned principally with the cultivation of maize. In 1604, Champlain reports that corn was being grown on the coast of Maine, where it grew to a height of 2 to 3 feet.[1] In 1614, Captain John Smith, describing Agawam (now Ipswich, Massachusetts), tells of many rising hills whose tops and sides were covered with corn fields and groves of trees.[2] While there are no reports on contemporaneous conditions in the Connecticut Valley, by 1635, when colonial settlements were being made along the river, maize was fast becoming an important food among the Indians, though probably not a staple. In his *History of Hadley*, Judd reports that at this time there were probably about seventy acres planted to maize by the natives from Holyoke to the Sugar Loaf Mountains. His statement that in most transfers of land the Indians reserved only the right to hunt and fish implies that the men were not concerned with maize cultivation.[3] They were, therefore, even at this late date, just food-gathering people at heart. Nevertheless, as stated by several writers, including Roger Williams,[4] maize was cultivated year by year by the women, who also watched over considerable stores of grain.

Before the prehistoric agriculture of these aborigines can be fully studied, it is necessary to determine what tools were used to cultivate maize. Three early commentators have given helpful information. Champlain speaks of a hoe made from the shell of the horseshoe crab and of a wooden spade (probably a flattened stick) used by the Indians of the Maine coast;[5] William Wood reports that he found some coastal Indians using clam shells for hoes;[6] and Roger Williams also speaks of the shell hoes, but says that formerly they were of stone.[7]

Williams' statement has led to an analysis of stone artifacts in an effort to determine which were hoes. While the shapes of stone hoes in widely separated sections of the country often resemble that of the metal hoe of today, it does not follow that this must be true of all hoes wherever found. It is probable that hoes have been developed by man all over the world to meet unrelated emergencies, and are, therefore, not the products of an associated industry. Hence, differences in soil and vegetation, as well as in individual tastes, might easily account for differences between hoes in widely separated localities.

As a result of the statement by Williams,

[1] De Champlain, 1604–1611, Vol. I, p. 102.
[2] Smith, 1614, p. 191.
[3] Judd, 1905, pp. 104–105.
[4] Williams, 1643, pp. 37, 38, 99.
[5] De Champlain, 1604–1611.
[6] Wood, 1634, p. 100.
[7] Williams, 1643, pp. 37, 38, 99.

several authorities have identified certain flat stone artifacts as hoes. About fifteen specimens from the Springfield district and twenty from the Northampton area of the Connecticut Valley belong in this class. Small numbers have been reported from Westfield and Hadley, and several were found in the Quabaug section in association with a supposed aboriginal corn field. These artifacts, usually made of thin slabs of basalt (trap rock), are 4 to 6 inches in length. They vary in width from 2 to 4 inches and are usually not over ½ inch thick. With a few exceptions, they have a fairly sharp cutting edge which in some cases has been ground smooth; in other cases, the grinding has not progressed so far. On the ground surfaces of many there is a luster which extends part way up both faces of the blade. Occasionally only one face is abraded. In some instances, the polish or patina, apparently the result of friction, has not destroyed the marks of percussion flaking, but appears within the grooves as well as on the ridges. These blades are usually quite flat with a more or less convex edge on one end. There is usually a taper toward the other end, sometimes terminating in an unworked point. At other times the taper ends at a squared-off base. Both sides are usually grooved or notched about midway between the ends—or sometimes a little nearer the tapered base—apparently for hafting. Occasionally only one side is so notched, the other being left unworked.

In his *Antiquities of the New England Indians*, C. C. Willoughby identifies these artifacts as hoe blades from the Connecticut Valley.[8] He ascribes the peculiar luster on the ground edge and portions of the adjacent surfaces to contact with the soil, but goes on to say that axes are found with this same luster or patina. His illustration of these blades suggests a possible method of hafting, that is, placing the base of the blade against the end of a crooked stick, the long end of which becomes the handle. The blade and stick are then bound together in some obscure way which the illustration fails to show.

These basalt blades are usually side-notched and were, therefore, undoubtedly hafted. Their resemblance to modern hoes should not be taken too seriously, however, for primitive agricultural requirements probably differed from those of today. This is suggested by an old photograph of a well-preserved corn field, reputedly Indian, found years ago in the vicinity of Northampton. It shows scattered hills of soil in which maize was presumably grown. In such a field the workers were probably chiefly concerned with digging holes only large and deep enough to receive, when needed, one or two small fish such as herring—as taught by Squantum at Plymouth[9]—in addition to two or three grains of maize and, sometimes, a few beans.[10] These holes were made in an area three or four feet across, which either was first made into a hill or was hilled up from time to time as the maize matured. The careful weeding of these hills testified to by early observers was probably done with hoes which, in the vicinity of Boston, were of clam shells, as reported by William Wood in 1634.

An illustration in J. F. Lafitau's *Moeurs des Sauvages Ameriquains*,[11] depicting corn cultivation among the Hurons of Canada, indicates similar practices. Four women are shown wielding hoes with pointed triangular blades, while four others appear to be dropping grains of maize into small prepared holes, several of which have been made in each of the hills shown. The hills are circular, about 3 or 4 feet in diameter and 4 to 6 inches high. There are eleven hills in the foreground, and others are apparently being made in the background by the women with hoes. This fine portrayal of Huron agricultural methods seems to agree with the picture of aboriginal agricultural practice in the Connecticut Valley already outlined. By permission of the Department of Mines and Resources, Ottawa, Canada, the writer has reproduced a section of the original Lafitau drawing (Fig. 4, *1*).

However, this system of planting in hills was probably found only in small fields of maize in historic times among the Indians of the Connecticut Valley, and may have been a development from a much simpler beginning. There are numerous reports of early observers that maize was grown in small private plots near most summer camps and villages, and it seems probable that this condition had persisted with little change since the introduction of agriculture into the valley. Digging sticks may have been used at first, since agriculture here was

[8] Willoughby, 1935, p. 171.

[9] Bradford, 1621, p. 29.

[10] De Champlain, 1604–1611, Vol. I, p. 101.

[11] Lafitau, 1724, Vol. II, p. 155.

undoubtedly derived from the Southwest, and digging sticks have been recovered archaeologically in that region.[12] Originally, cultivation was probably limited to making holes for seed, and the more advanced methods of a later day were doubtless lacking. The many open fertile plots on the borders of the forest would produce successful maize crops with little working of the soil other than making holes. The removal of small brush, undoubtedly a frequent necessity, was sometimes accomplished by burning. Later, with the development of more extensive planting areas, soil-breaking hoes were probably used.

Any attempt to identify implements used in cultivation must be in terms of this agricultural setting. For several reasons it seems improbable that the artifacts which Willoughby considered to be hoe blades were really made primarily for this purpose. (1) Most of them, weighing as they do only about a half pound, would seem too frail to serve for breaking soil, and their blades are too wide for digging the small holes required for planting. (2) Their cutting edges are ground apparently with a hard, flat-surfaced stone, probably not by soil abrasion. In many cases, the effects of the original flaking have been ground away. In actual tests, the writer found that this could be done with an abrading stone but that use in the soil produced no abrasive effect. The grinding process takes several hours under favorable conditions, an effort which seems unnecessary for a tool used to cut a substance as soft as soil. The patina ascribed by some to soil abrasion is produced as well by friction against any dense object such as wood, as the writer has discovered by experiment. (3) The presence of the side notches implies that they were essential to hold in place the thongs securing whatever haft was intended. However, when these blades are placed at the butt or under the crooked end of a stick, as they would be were they hoes, it will be found they may be securely attached without the assistance of such notches. The haft suggested by Willoughby seems improbable because of its unnecessary complexity. (4) Finally, very few of these blades occur today on the several hundred habitation sites in the Connecticut Valley. The writer has found but two fractured specimens during examinations of the sites covering several years, and only fifty or seventy-five perfect blades are on display in public and private museums in the district. It has been suggested that, although a few may have been used as hoes, most of these implements were probably small axes. The hafting of an ax would require the side notches, and the uses to which an ax is put would account for their sharpened edges. Since it has been reported that in an actual test one of them, in a side haft, proved to be an excellent tool for cutting small brush, these blades may have been used for clearing garden plots preparatory to planting. They may also have been used to cut corn stalks, which are known to have been used for thatching huts and weaving mats.

If these blades were hoes, their paucity would suggest that cultivation of maize at the three hundred or more sites in the valley was almost nonexistent. But it is known that this was not the case, so other agricultural tools were undoubtedly used. These must have been either digging sticks of perishable material or stone hoes differing from those artifacts generally accepted as hoes in the past. Those who accept the latter alternative—among them the writer—believe that the prehistoric stone hoes mentioned by Williams were distinctive, specialized implements unlike the Willoughby "hoes." The fact that on every important camp site in the Connecticut Valley there occur percussion-flaked stones, some more carefully worked than others, which seem well adapted to the type of primitive agriculture previously outlined has prompted research along a new line. Because of certain consistent characteristics of these artifacts, it has been possible to classify them in two groups, each based largely on shape. In one group the trait common to all specimens is the presence of a tang, while in the other it is a triangular form. Specimens of both types vary in weight, size, style, and kind of stone used.

The tanged type (Fig. 4, *9–11*) includes ten specimens varying from 2½ to 4½ inches in length and width. The blade ranges between about ¼ inch and 1¼ inches in thickness at the center and is gradually thinned down to the edges by percussion-flaking. Small blades are about as numerous as large ones. Every specimen has a perceptible conchoidal hook, and a prominent tang, apparently designed as a shank for attachment to the handle, is always present. Some have straight edges, others have spoon-like blades resembling stone hoes from New Jersey. Indigenous stones—quartzite,

[12] Keur, 1944.

hornblende, and basalt—were used. It is evident that the makers of these implements utilized spalls with conchoidal fractures, whenever available, to provide the hook which gave the hafted tool such a tilt that a crooked stick was unnecessary for a handle. The back of the tang was probably placed against the surface of a straight stick and bound to it with leather thongs. The curve of the stone would put the blade and handle at an obtuse angle, which would seem to be about right for a hoe meant to dig holes or break soil. The creation of a more acute tilt, when required, could be accomplished either by cutting a sloping notch in the side of the stick to receive the tang or by inserting a frog of wood beneath it before binding.

The forty-three specimens comprising the triangular type (Fig. 4, *4–8*) vary in length from 2 to 7 inches. Small ones 2–3½ inches long are much more numerous than larger ones. All are percussion-flaked from indigenous stones, including quartzite, basalt, granite, shale, quartz, and sandstone. They are either equilateral or elongated. Sometimes the two base points have been intentionally removed, sometimes they are rounded; the third point, flattened, generally blunted, and always well shaped, is probably the blade. A most significant trait is the oblique base, sometimes widely notched as though to furnish a seat for the handle. This base varies in thickness from ½ to 2 inches. Implements of this type are also often made from spalls with conchoidal fractures, so that they are curved from end to end and would require for handles nothing more elaborate than straight sticks which might be picked up in the forest. When the oblique base of the artifact is placed against such a stick, the blade tilts at the obtuse angle required for digging holes. The pointed blade quickly makes a hole in soil that is hard or in which loose stones are present, perhaps accounting for the fact that it occurs more frequently than other types. Another reason for its evident popularity may be that most specimens require no notching of the handle as a preliminary step in hafting. A twist or two of the thong around the end of the stick provides the required support. Most blades were probably bound as shown in Figure 4, *2*, *3*. The front and back views show that the thongs are wound around the sides of the blade and under the stick, before and behind, in a criss-cross pattern. Especially where the blade has rounded base points and no notches (Fig. 4, *5*), this method of binding enables the thongs to hug the stone closely and thus prevent slipping.

Another artifact which appears to be an agricultural tool occurs infrequently; only nine specimens have been found. Though somewhat variable, they are usually flat and fairly large—4–7 inches in length. The blade is either pointed or straight-edged. They cannot be fitted into the hoe typology because they lack consistent hafting traits. Furthermore, while they may be attached to the butt of a stick for use as hoes, they lack the rigidity necessary for the heavy work for which their size should fit them. The use of a stick with a crook at an obtuse angle for a handle would correct this weakness, but such a haft would be impossible when the blade has a pointed base. The considerable wear across the back and near the base of one specimen is such as would develop if a straight stick, bound with thongs against the side of the blade, were to work loose to permit play between handle and blade. An implement so hafted would closely resemble a spade. However, until further evidence is produced, the identity of these blades will remain problematical.

The foregoing typology is based on well-shaped artifacts found by the writer on Connecticut Valley sites during several years of research. Numerous other specimens, more roughly worked, were found but were neither saved nor tabulated. Of the specimens collected, those with tangs were from nine habitation sites, while triangular ones were surface finds from twenty-five sites. The greatest number found on any one site—eleven triangular blades—came from M-18-13. Specimens of the triangular type are about four times as numerous as those of the tanged type. Every return search of the camp sites yields new specimens, which suggests that these blades were in common use. In fact, it may be safe to postulate that there are hundreds on a large number of sites still to be recovered.

SUMMARY AND CONCLUSION

On the basis of the evidence presented in this paper, it seems probable that stone hoes and possibly spades were used in the agriculture of the Connecticut Valley of Massachusetts. The origin and development of these tools involve questions which fire the imagination of all those concerned with the subject. The artifacts

classified as hoes in this paper do not in themselves give the answers, but the remarkable consistency with which they exhibit numerous specialized hoe traits seems to indicate a homogeneous development during the agricultural stage, and furnishes a basis for a search into foregoing stages for the answers.

It is generally agreed that the period when steatite was quarried and fashioned into bowls, cups, and plates probably preceded the introduction of maize. With this as a premise, certain observations at two local prehistoric steatite quarries may have a bearing on the origin of agricultural tools. During excavation of the Westfield quarry, an artifact 6½ inches long and 5 inches wide was found in situ among the tailings and near a steatite outcropping. The implement is somewhat conchoidal in shape, with a tang ¾ inch thick and a blade that tapers to ¼ inch at the edge. It is made of a hard mica schist, and shows wear at both ends. It bears similarities to the tanged hoe, but it has only a slight hook and was probably hafted with a crooked stick over the base. An artifact of quartzite 6 inches long by 4 inches wide was later found in disturbed tailings on the same site. Curiously enough, it is percussion-flaked into a shape much like that of the triangular hoe. Its base, 2 inches thick, is oblique, and the blade tapers to a thin edge. The flat, pointed blade is elongated, and the two base points have been intentionally removed. It could be hafted without difficulty on a straight stick like the triangular agricultural hoe, and it probably was used like a hoe for loosening and removing steatite tailings.

Finds at the North Wilbraham steatite quarry include an implement resembling the tanged hoe, two large triangular hoes of granite, and flat stones some of which had been chipped into a more or less oval shape with notches near the base, apparently for hafting. The latter, which measure about 5 by 6 inches, probably were spades used to fill baskets with quarry tailings for disposal.

While all these quarry tools are rather crudely shaped, they are very similar to native agricultural hoes and spades. Might they not be the prototypes of the cultivating tools that followed? If so, the steatite industry was the testing laboratory from which agricultural tools eventually emerged.

Stone hoes were evidently developed by peoples of inland districts, where the large clam shells of the coastal regions were not available. However, it is interesting to note that the curved shape of the Connecticut Valley hoes closely resembles that of a shell.

While none of these hoes is ground or polished, there is frequently wear along the edge of the blade. Because of the short planting seasons and the ease with which new tools could be made from the common native stones used, hoes were probably not used long enough to develop more than a superficial soil abrasion. The practice of making a hoe as needed on or near the scene of planting, from any suitable native stone, seems to rule out Willoughby's identification of the basalt blades previously discussed. The manufacture of these problematical artifacts must have required skill and time, and it seems unlikely that they would have taken the place of such a popular tool as a hoe. If they were hoes, it is difficult to explain their paucity on all camp sites, since it is known that maize was grown universally throughout the valley. Many hundreds of hoes would have been required for the cultivation of the hundreds of maize plots, even allowing for possible duplications from season to season. So for the present, until more convincing evidence is available, these rare blades probably should be excluded from this category.

Although the tools classified here as hoes are specialized, well-defined types, the percussion-flaking which shaped them is often irregular and crude. It seems to be the work not of skilled toolmakers, but rather of unskilled fingers which have been guided, however, by set standards of form. It is as if the hoes were made by individuals—probably the agricultural workers themselves—who had neither the time nor the specialized skill in flaking to produce a finely shaped artifact, but who knew from experience that a roughly flaked hoe with a properly shaped blade would do as much work and do it just as well. The frequent use of shale schists, usually for the smaller implements, lends weight to this hypothesis, for it is difficult to believe that a professional artifact-maker would select such soft and friable material. It seems probable, therefore, that the women, the tillers of the soil, made most of the hoes. Although occasional large hoes of 2- to 5-pound weight which occur in association with habitation sites may have been made and used by the men for special purposes such as digging graves or breaking rough ground preparatory to settle-

ment or planting, most of the specimens seem to be the small planting hoes presumably used by the women.

Evidence to support this hypothesis has been observed at Mount Tom in the Manhan River Valley. On site M-18-185, at a point where the northern rocky slopes at the foot of Mount Nonotuck change into smooth tillable soil, there were found on the surface four triangular granite hoes 3 to 5 inches in length, two of them shown in Figure 4, *4, 5*. They were found in the corner of a present-day corn field, near which there is rising ground covered with a glacial deposit of loose granite and quartzite boulders. Much of the granite is fragmentary, and some of it is water-eroded. The drift granite lying close to the site includes stones of the same texture as that of the stone from which the hoes are made. As no fire stones appeared in association with the implements, it is probable that the site was a small maize plot isolated from any established camp. This evidence strongly implies that workers, probably women, came here to cultivate and made planting hoes from the granite at hand.

Lafitau's illustration (Fig. 4, *1*), although depicting a scene at a date later than that of the stone hoes of the Connecticut Valley, concerns a people who had probably changed but little as a result of limited colonizing contacts and whose customs were no doubt a survival of those of their ancestors. Moreover, since the Hurons depicted had for many years been friendly neighbors and allies of the Algonkians,[13] the stock to which the River Confederacy of the Connecticut Valley is thought to have belonged, it is quite likely that the agricultural development of both peoples was closely related. The photograph of a reputed Indian cornfield at Northampton suggests a method of tillage similar to that of the Hurons.

On logical grounds, it seems likely that the people of this valley, like the Hurons, used the triangular hoe in preference to the tanged type. Large hoes of both types, 4–6 inches in length, may have been used to break up the soil and to make the hills. Spades of large flat blades of various shapes attached to straight sticks may also have been used for this work. The digging stick may have been used originally to make holes for the seed, but later two types of small planting hoes, 2–3½ inches in length, were developed for this purpose. These hoes, especially the triangular type, may have been developed as a result of efforts to improve the digging stick by giving it a durable point of stone.

These hoes, especially those of the triangular type, occur with great frequency on most habitation sites. Their often elongated blades are well suited for digging the small holes required for planting, or holes deep and large enough for fish fertilizer, when the soil required it. The digging stick may have proved inadequate for such work in this region, so that the development of a more efficient stone tool was necessary. Finally, the weeds that came with every planting could have been rooted from among the maize shoots with these small hoes without the danger of disturbing the crop.

In conclusion, the evidence indicates that the agricultural tools of the Connecticut Valley are not, as some suggest, the finely worked artifacts, probably made by experienced artisans, but are, rather, roughly made artifacts of clear-cut, functional types. Their identification is convincing only when the various possibilities are evaluated in terms of the best evidence as to the aboriginal agricultural conditions of the locality.

[13] Jenness, 1934, p. 1.

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Holyoke, Massachusetts
June, 1945

A GRAPHICAL APPROACH TO SOME PROBLEMS IN MAYA ASTRONOMY

ROBERT H. MERRILL

ALTHOUGH graphical methods are in common use by engineers and astronomers, they are less familiar to archaeologists. To portray the rhythm of celestial motions, astronomers often use "elongation" diagrams to show angular distances of moon and planets from the sun on each day of the calendar year.

The Maya recognized a zone within which the planets could not be seen because of proximity to the brilliant disk of the sun. The moon cannot be seen on the day of conjunction with the sun, and is generally invisible on the day before and the day after this "new moon day." The three consecutive days in the lunar table of the Dresden Codex may represent the 3-day phase of moon disappearance. Their Venus table records the period of Venus disappearance as 8 days; this was the only phase which could be precisely determined to the nearest day by observation.

In the 1930 elongation chart (Fig. 5) the more important hours near sunrise and sunset are shown in larger scale than the less important ones near midnight. Thus we read that Mercury was in that year morning star until January 18. After 8 days of disappearance it was evening star from January 26 to March 21. It disappeared for 19 days while passing through superior conjunction, but on April 9 it again emerged as morning star, remaining visible for 36 days until completing the first cycle on May 14. The successive intervals 8-54-19-36 of cycle 1 were counted 11-44-19-36 in cycle 2, and in cycle 3 were still different. To generalize from such irregular motions may have baffled the Maya astronomers, but in the case of Venus they evolved the formula 236-90-250-8, the average total being 584 days.

On opposite sides of the following table are recorded two ways of viewing the Venus cycles, namely, in the plane of the ecliptic and in the plane of the equator, the differences being negligible.

A few years ago, Dr. Curtis related, in *Popular Astronomy*, how, without optical aid, he was able to see Venus every day through the inferior conjunction passage of September, 1935, and later through the superior conjunction of June, 1936. Conditions then were unusually favorable. He used building shadows to hide the sun and knew precisely where to look. There may have been equally keen observers among the Maya, for they certainly knew that the morning and the evening stars were one and the same object. The Greeks may not have appreciated this when they named the morning apparition Phosphorus and the evening star Hesperus.

Why, then, did the Maya count as decidedly unsymmetrical the three phases we regard as symmetrical? Possibly the morning haze retarded early visibility, while the relatively clearer sunsets enhanced the evening visibility. They probably knew that the winter half year, 179 days, was 7 days shorter than the summer half year of 186 days, and they may have regarded 7 as a mystic number.

INTERVALS IN DAYS BETWEEN VENUS CONJUNCTIONS

The mean motion intervals customarily quoted are 583, 587, 580, 587, 583 repeated. Almanac data are given in the plane of the ecliptic and tabulated on the left in the table on page 38. The same have been plotted in the plane of the equator, and the scaled results appear at the right. Superior conjunctions are denoted by "S" and inferior conjunctions by "I."

Having allotted 8 and 90 days to measure the periods of invisibility, there remained $584-98=486$ days for the two periods of visibility, which, if equal, would have given 243 days to each. Making allowance for the above inequality they arrived at $243-7=236$ days for the phase of morning star, and $243+7=250$ days for the phase of evening star. At any rate, their formula was 236, 90, 250, 8 for the four divisions of the 584-day mean Venus period.

The wide range of 16 days between superior conjunctions passed unnoticed because not directly observable. Only the 8 days of disappearance could be located to the nearest day, and five successive cycles measured 583, 587, 580, 587, 583, each fairly close to the average of 584 days. Counting inferior conjunction as the midpoint, 292, the zone of invisibility extended from day 288 to day 296. The formula used by the Mayas located the beginning of the evening star period as $288-250=$ day 38, and the end-

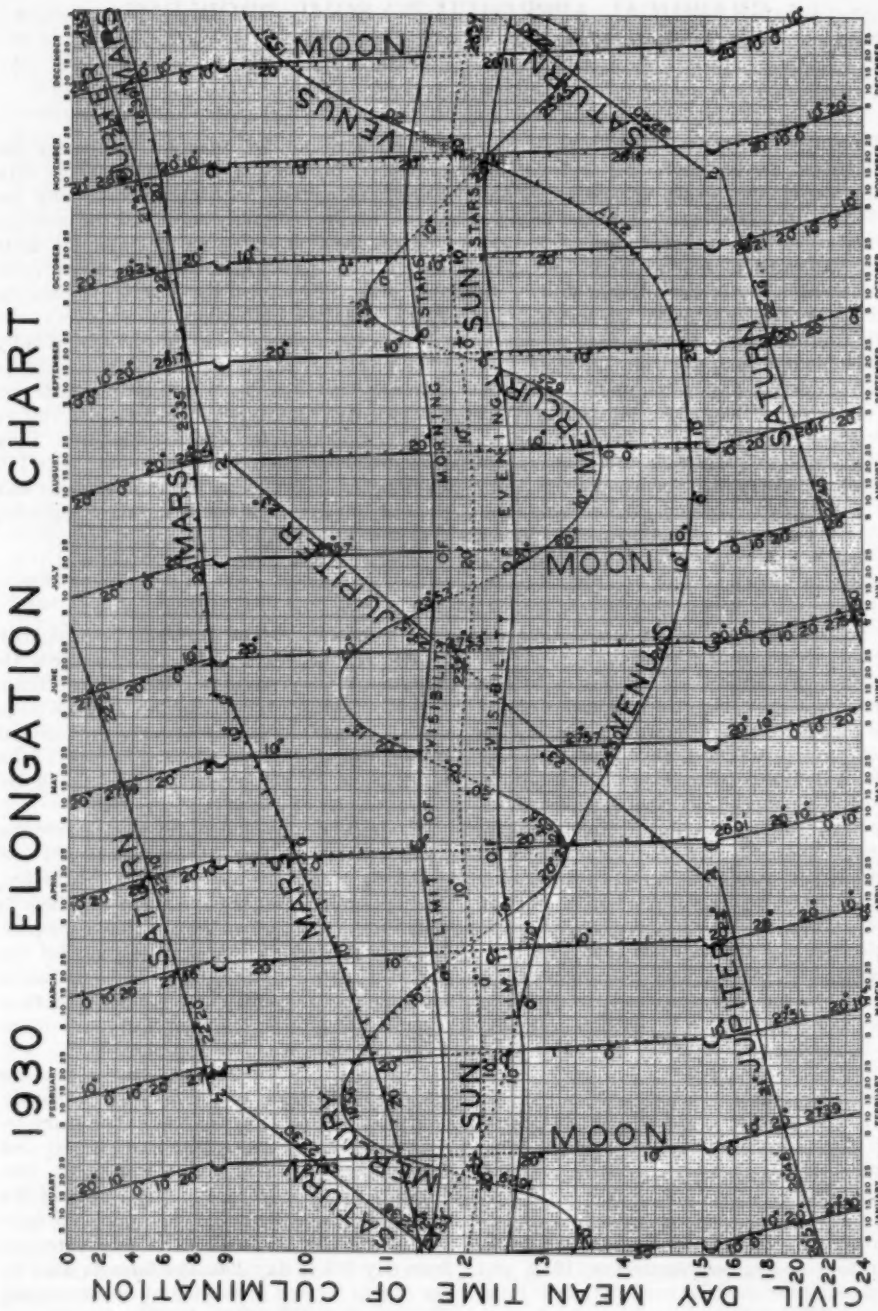


FIG. 5. Elongation chart for the year 1930.

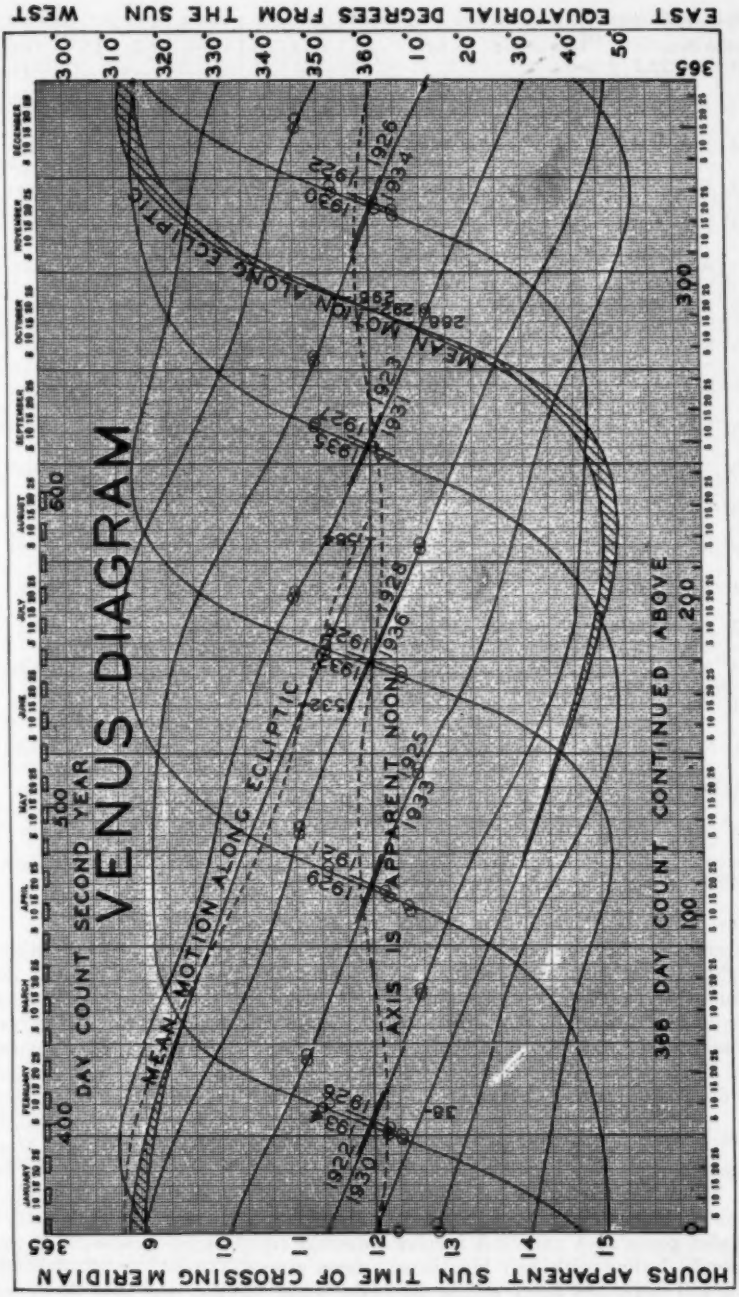


FIG. 6. ILLUSTRATING CELESTIAL RECURRENCE PATTERNS.

FIG. 5. Elongation chart for the year 1930.

Almanac Data on the Ecliptic						*Mean*					*Scaled Data on the Equator*	
Conjunction date			I-S	S-I	S-S	I-I	I-I	I-I	S-S	S-I	I-S	Date Conj.
I	1921	Apr. 22.2										Apr. 18 I
			292.6								294	
S	1922	Feb. 8.8				581.6	583	583				Feb. 6 S
				289.0						289		
I	1922	Nov. 24.8			578.2				580			Nov. 21 I
			289.2								291	
S	1923	Sep. 9.9				584.2	583	588				Sep. 6 S
				295.0						297		
I	1924	Jul. 1.0 leap yr			592.5				592			Jun. 30 I
			297.5								295	
S	1925	Apr. 24.0 civil day starts				587.1	587	585				Apr. 21 S
				289.6						290		
I	1926	Feb. 7.6			576.5				577			
			286.9								287	
S	1926	Nov. 21.5				580.2	580	579				Nov. 19 S
			293.3							292		
I	1927	Sep. 10.8			588.1				590			Sep. 7 I
			294.8								298	
S	1928	Jul. 1.3 leap yr				587.6	587	589				Jul. 1 S
				292.8						291		
I	1929	Apr. 20.4			585.1				584			Apri. 18 I
			292.3								293	
S	1930	Feb. 6.7				581.4	583	582				Feb. 5 S
				289.1						289		
I	1930	Nov 22.8			578.4				577			Nov. 21 I
			289.3								288	
S	1931	Sep. 8.1				584.4	583	585				Sep. 5 S
				295.1						297		
I	1932	Jun. 29.2 leap yr			591.6				592			Jun. 28 I
			296.5								295	
S	1933	Apr. 21.7				586.0	587	585				Apr. 19 S
				289.5						290		
I	1934	Feb. 5.2			576.3				577			Feb. 3 I
			286.8								287	
S	1934	Nov. 18.9				580.1	580	579				Nov. 17 S
				293.3						292		
I	1935	Sep. 8.3			588.4				589			Sep. 5 I
			295.1								297	
S	1936	Jun. 29.4					587					Jun. 28 S

Note that intervals between superior conjunctions vary from 592.5 to 576.5, a maximum range of 16 days, as against 7 days calculated from mean motion at inferior conjunction.

ing of the morning star period as 296+236=day 532. We might have reckoned the 90 days of invisibility from 45 days before to 45 days after superior conjunction; but the Maya had no means of observing the precise date of superior conjunction. They set the limits at 52 days before and 38 days after superior conjunction.

THE MAYA MOON EPHEMERIS

The corrections marked "E" on the moon chart (Fig. 7) are emendations generally accepted to make the manuscript numbers consistent. Dresden pages 51-A and 52-A involve 15-day intervals dealing with half lunations, new moon to full moon and hence eclipse distances. Few eclipses were visible to the Maya, but their moon-age record involved almost continuous adjustments. Dr. Teeple notes that these moon numbers exceed all other astronomical data now available from the Maya area.

The sixty-nine successive columns occupy two lines across the eight Dresden pages, but presentation in thirds favors symmetry. At the end marked by "Picture" 10, an additional day must be counted from terminal 11 Manik to reenter at 7 Chicchan in column 1. Two table lengths, 2×405=810 moons, probably involved 25 full days additional to maintain the 29.5-day average count by blanced pairs. As separate 6-moon groups, the 8.17 or 177-day length was the same whether the long 30 occupied the "odd" 1, 3, 5 positions or the "even" 2, 4, 6 po-

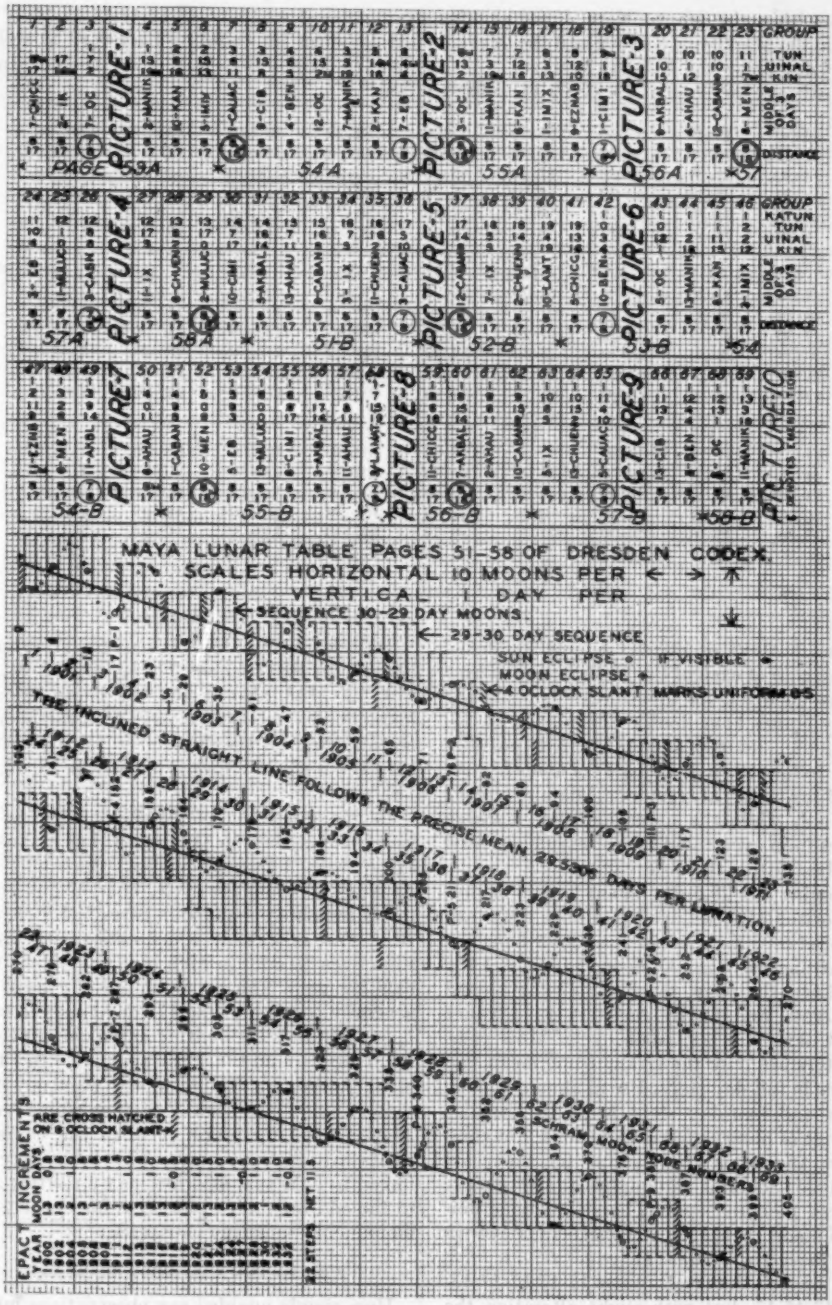


Fig. 7.—Moon Chart.

sitions within the group. Note, however, the "odd" order starts ½ day above and the "even" order, ½ day below the 29.5-day average.

The average for 5 moons $5 \times 29.5 = 147.5$ days

	1–30	19–29	37–30	55–29	73–30	91–29	109–30	127–29	145–30$_9$	
	2–29	20–30	38–29	56–30	74–29	92–30	110–29	128–30	146–30	
	3–30	21–29	39–30	57–29	75–30	93–29	111–30	129–29	147–29	
	4–29	22–30	40–29	58–30	76–29	94–30	112–29	130–30$_8$	148–30	
	5–30	23–29	41–30	59–29	77–30	95–29	113–30$_7$	131–30	149–29	
	6–29	24–30	42–29	60–30	78–29	96–30	114–30	132–29	150–30	
	—	—	—	—	—	—	—	—	—	
Group	177	177	177	177	177	177	178	177	178	Totals
	7–30	25–29	43–30	61–29	79–30	97–29	115–29	133–30	151–29	
	8–29	26–30	44–29	62–30	80–29	98–30$_6$	116–30	134–29	152–30	
	9–30	27–39	45–30	63–29	81–30$_5$	99–30	117–29	135–30	153–29	
	10–29	28–30	46–29	64–30	82–30	100–29	118–30	136–29	154–30	
	11–30	29–29	47–30	65–30$_4$	83–29	101–30	119–29	137–30	155–29	
	12–29	30–30	48–29	66–29	84–30	102–29	120–30	138–29	156–30	
	—	—	—	—	—	—	—	—	—	
	177	177	177	177	178	177	177	177	177	
	13–30	31–29	49–30$_3$	67–30	85–29	103–30	121–29	139–30	157–29	
	14–29	32–30$_2$	50–30	68–29	86–30	104–29	122–30	140–29	158–30	
	15–30	33–30	51–29	69–30	87–29	105–30	123–29	141–30	159–29	
	16–29	34–29	52–30	70–29	88–30	106–29	124–30	142–29	160–30	
	17–30$_1$	35–30	53–29	71–30	89–29	107–30	125–29	143–30	161–29	
	18–30	36–29	54–30	72–29	90–30	108–29	126–30	144–29	162–30	
	—	—	—	—	—	—	—	—	—	
	178	177	178	177	177	177	177	177	177$_{10}$	

but the 7.8 Maya or 148-day groups result in a half-day gain at each of the 9 "Pictures." These are here encircled for identification, while the 178-day groups have double circles at 7, 14, 23, 29, 37, 52, and 60; a double circle possibly should follow 69, if it is used for re-entry. At each of the latter, at least a half-day increment is located, while the other half-day marking changes from even to odd order may be located where best fitting the observations.

Assuming the 29.5-day moon length to proceed horizontally, we need plot only the mean increment 0.0306 days per lunation. Thus the straight-line mean in 135 moons advances 4.135 days down the page, reaches 8.27 the second time across, and finally registers an advance of 12.4 days in a 405-moon cycle. Most commentators use only the straight-line mean and advance in full-day steps. Use of almanac moon lengths and half-day steps helps to visualize the range of observational variations. For comparison, the moon chart shows for the years 1900–1933 Schrams Mondphasentafel eclipse numbers, modern "epacts," and Palenque uniform 6-moon groups, as suggested by Beyer in *Maya Research*, Volume II, pages 66–71.

Dr. Beyer's intervals were 17-15-17-17-15-21-11-17-15-17, but with slight alteration the symmetrical 17-15-17-15-17-17-15-17-15-17 order is better for counting backward as well as forward. His tabular values are then arranged thus:

Half-day gains at 30-30 sequences are numbered 1 to 10 at moons 17, 32, 49, 64, 81, 98, 113, 130, 145, also at $162 = 0$. These half-day increments agree more closely with mean moon motion than do the customary full day steps, here about equally spaced at months 18, 54, 84, 114, and 150.

In five repetitions of the above schedule the gain is 25 full days in 810 moons. The end result agrees with 2 Dresden cycles, which gain 12½ days in 405 moons per cycle. The 177-day group totals remain unchanged in cases 2, 4, 6, 8, and 10 in spite of a 30-30 which changes the order from even to odd, in each case equivalent to a half-day gain. The apparent full-day steps are generally counted at the 178-day totals in cases 1, 3, 5, 7, and 9. Counting actual inscriptions, Dr. Teeple found that "the odds are 3 to 1 that glyph A shows a 30-day moon, when glyph C shows an odd coefficient." Similar odds favor a 29-day moon for glyph A when the C coefficients are even numbers. The table may be forced to fit actual inscriptions by shifting 30's to the odd-numbered places; unfortunately the monuments sometimes disagree.

A MAYA SUN CALENDAR DIAGRAM

How simple unaided eye observations might

accurately establish the true length of the tropical year to the nearest day has been described on pages 307–311 in the January, 1945, number of AMERICAN ANTIQUITY. Figure 8 pictures to relative scale a series of sunset marks for the three days nearest the equinoctial points. The six consecutive years include two leap years and three intervening "common" 365-day years. For each calendar date the six observed points are joined, forming a zigzag line. The several measurements north and south from the east-west equinox line were:

Year	March 21	March 22	Sept. 23	Sept. 24
1939	9 N	8 S	16 S	1 N
1940	4 S	20 S	3 S	13 N a leap year
1941	0	16 S	7 S	9 N
1942	4 N	12 S	11 S	5 N
1943	8 N	8 S	15 S	1 N
1944	5 S	21 S	3 S	14 N

We therefore conclude that the annual shift is 4 feet farther north each year in March but 4 feet farther south in September for each of the 3 common years. On leap years, the shift is 12 feet back again to the starting point.

The winter intervals, from September 23 of one year to March 22 of the year following, are indicated by dotted lines. When numbered continuously from January 0, September 23 is day 266 and the following March 22 is day 446. Hence, the winter interval is $446-266=180$ days. This is an even one-half "tun" and into it exactly 9 Maya 20-day months may be fitted.

Although the precise means, 178.811 and 186.431, for winter and summer half years were unknown to the Maya, they may have adopted the even numbers 180 and 185 and adjusted their calendar accordingly.

Figure 8 shows how at Macatawa, a harbor on the east short of Lake Michigan, it is easy to determine the true equinox line by the mariner's rule "when the center of the sun's disk is a a distance above the water horizon equal to its own diameter, it is then almost exactly in the true horizon." This obviates the necessity of making corrections for dip and refraction and lessens interference from clouds which frequently obscure the water horizon.

Conditions at Copan, where there is no water horizon, were further simulated by using the top of the beacon as an additional sighting datum, and after applying a small constant correction it was found that the same uniformity marked the final results. At uniform 20-day intervals from the equinox, sunsets crossed the "astronomical base line"; and at 40-day intervals from the equinox, the "Z" days, when the sun crossed Copan's zenith, could be observed accurately, also to the nearest day. The autumnal "Z" day may well have been New Year day, 0 Pop, when the Maya day count began, if the account of Pio Pérez can be trusted.

In our modern corrected Gregorian calendar, the above Copan dates are now fixed at September 23, March 22, April 11, May 1, and August 14; but the "Z" day in Landa's old-style calendar was August 4. Probably in A.D. 1553 Landa

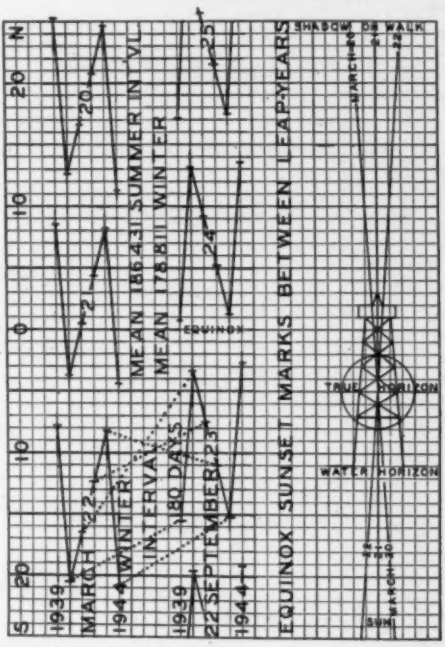


FIG. 8.

recorded July 16 as marking the first of Pop; the year end, 0 Pop, was then July 15, old style. Accidental lack of understanding resulted in fixation thereafter of Julian and Mayan calendars as marked along the bottom line of Figure 9. From the theoretical starting point, August 4, the terminal date, 0 Pop, had advanced just 20 days to July 15 at the rate now known to be 0.2422 days per year. Hence "Z" day fell on 0 Pop $20\div0.2422$ or 82.6 years prior to 1553, namely, in A.D. 1471, whereas Dr. Spinden's Table XV gives A.D. 1464, 40 B.C., 1548 B.C., and 3056 B.C. Recalculated at 1507-year inter-

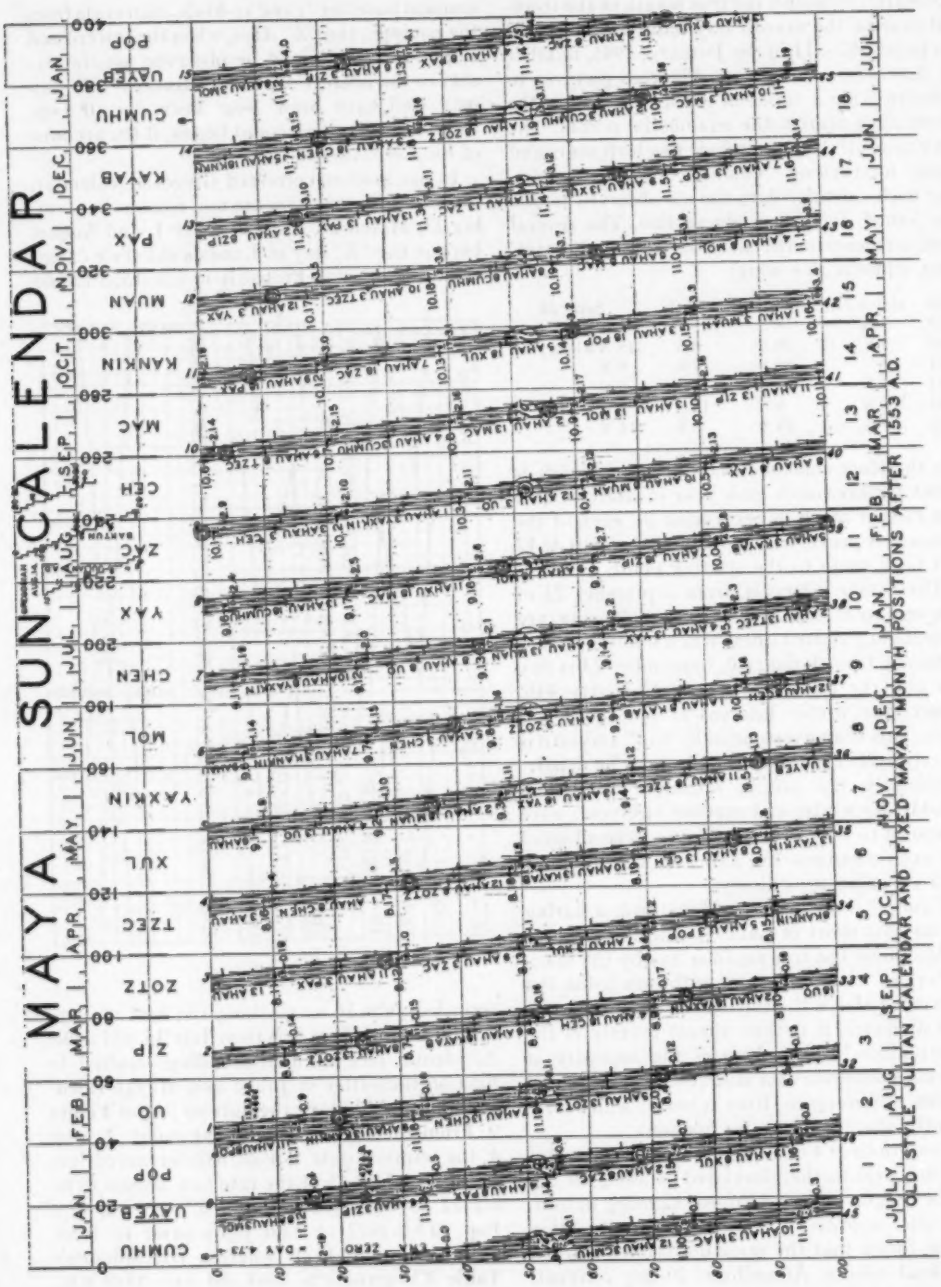

MAYA SUN CALENDAR
OLD STYLE JULIAN CALENDAR AND FIXED MAYAN MONTH POSITIONS AFTER 1553 A.D.
JULY AUG SEP OCT NOV DEC JAN FEB MAR APR MAY JUN JUL AUG SEP OCT NOV DEC JAN FEB MAR APR MAY JUN
POP UAYEB CUMHU ZERO DAY 4.73 = 11.12.2-0
CUMHU UAYEB POP UO ZIP ZOTZ TZEC XUL YAXKIN MOL CHEN YAX ZAC CEH MAC KANKIN MUAN PAX KAYAB CUMHU UAYEB POP

FIG. 9.

vals, these dates when "Z" day fell on 0 Pop were A.D. 1471, 36 B.C., 1543 B.C., and 3050 B.C., respectively. A new diagram, Figure 9, now replaces the Figure 26 in my article cited above which was erroneously based on Dr. Spinden's Table XV.

Before photographic reduction, Figure 9 had a horizontal scale of 20 days to the inch, and a vertical scale of 10 years to the inch. Unfortunately, the small unit square rulings fail to show, but the artibrary numbering from 0 in the upper left corner is marked off along the top margin from zero to 400 days, and in even decades downward along the left margin from 0 to 100 years. The 18 Mayan 20-day months named, from Pop to Cumhu, are also numbered consecutively 1–18 at the bottom of each monthly column. Thus the irregular ¼-month Uayeb falls on the left of day 20 in column 0, and to the right of day 380 in column 19.

This arrangement, like that of the Mayan tables, facilitates counting both backwrads and forwards in units and fives. Terminal margins are continued from the opposite margins by advancing one line or column, making spiral lines on a cylindrical diagram. The century completed in the bottom line is continued immediately above in the top line. Each horizontal line marks off the 365 days of the Maya "vague" year, each followed by the succeeding line next below.

Dr. Spinden's Table XVI is reproduced graphically along the top line, where day numbers from January 0 to December 31 of the Julian calendar may be read off directly. For scaling, an additional strip is easily graduated from August to August on the same section paper and cut apart for use as a movable measuring strip.

In the Gregorian calendar, the location of "Z" day is fixed at August 14, but in the old style Julian calendar, "Z" day fell on August 4 in the sixteenth century and advanced by one-day steps to September 9 at century 31 B.C., as shown at top center in Figure 9. Because Maya days are counted in corresponding Julian day numbers, allowance for the Julian seasonal error may be important.

It is convenient to assume as arbitrary datum for numbering "vague" years one of those periods when "Z" day fell exactly on 0 Pop. This recurred at intervals of 1508 vague years, equal to 1507 tropical years. Thus, calling 0 Pop in the top line Maya 365-day vague year number 0, "Z" day again falls on the vertical line between 4 Uayeb and 0 Pop at vague year number 1508, again at 3016, and again at 4524, which is the particular 0 Pop cycle of 1471 A.D., as figured backward from the Landa correlation date of 1553.

Each traverse of a century from top line 0 to bottom line 100 marks a "Z" day advance of 24.22 days. The first oblique "Z" date line extends from 0 Pop to 4.22 Uo, the second century from 4.22 Uo in the top line to 8.44 Zip, etc. The centuries of the first 0 Pop cycle are numbered 0 to 15 at the top. The second cycle numbers 15 to 30 have been omitted to avoid congestion. The third cycle and beginning of the fourth, spanning the historic period, are numbered along the bottom as centuries 32 to 46.

The day count in 360-day "tuns" has also been graduated along the same spiral "Z" date line. Baktuns 0 to 4 appear on the right, baktuns 4 to 8 appear centrally, and baktuns 8 to 12 are numbered on the left of spiral 3 and beginning of spiral 4. Each spiral is displaced 8 years vertically and 1.936 days horizontally from the preceding. The numbered katuns are graphically subdivided at tuns 5, 10, and 15; and from these marked points single tuns are estimated as 360/365 of a ruled space. Katun-ending dates are written along the left spiral only.

The Landa correlation date has been accepted, and the conflicting Bacalar statement is explained as follows. There is a difference of 6 days in the Julian and Mayan date intervals, explained by Martinez as neglect of the 3 extra days in leap years 1544, 1548, and 1552. His 3-day shift accounts for half the discrepancy at the cost of being "3 days short of all the new moons recorded by the Maya." As an alternative, let us suppose the same attempted 3-day adjustment was mistakenly made in the wrong direction. Probably no mistake was made in the Maya calendar calculations, and the interval from 11 Chuen 18 Zac to 1 Pop was correctly counted to July 16, 1553, as 9 Maya years and 148 days. July 16 is day 197, and if it were figured that 148 plus 3, or 151 days, should be taken from 197, the result would be 197−151, or day 46, which was February 15, 1544, the date reported. Instead, the Julian 9 years included 3 extra days, and there remained 148−3, or 145 days, to be deducted; and 197−145 is day 52, which is February 21. If February 15 is

FIG. 9.

replaced by February 21, the two dates are consistent and Landa is confirmed.

Instead of the sixteenth century Landa date, 12 Kan 1 Pop, the ancient usage tabulated by Goodman shows 12 Kan 2 Pop in his year pattern #49. Counting back 82−52 or 30 numbers gives #49−30 or pattern #19. Years 0, 1508, 3016, and 4524 therefore have pattern #19, and so have all calendar rounds distant even multiples of 52 years thereform. For the cycle spanning the historic period the calendar rounds are numbered 0 at 3016 and 29 at 4524, and these numbers are encircled for identification. Each of these calendar rounds so numbered has year pattern #19, and the pattern number of intermediate years is thus easily counted.

ing the length of the diagram the Julian calendar advances 1 Mayan year, for 1507 Julian years just equal 1508 Mayan vague years.

The calculation of sun advance through the vague years at any date, which Dr. Teeple has named "determinants," may now be directly measured graphically to the nearest day in original Mayan units, without conversion into decimal numbers and Julian dates, and reconversion back into Mayan terms. "Anniversaries" may be of several lengths in days.

Long count Initial Series numbered along the spiral date lines in tuns have 360 days.

Maya calendar vague years fix the chart background in cycles of an even 365 days.

The calendar position of "Z" day is marked

Christian calendar year	Old style date	Year day number	Julian Day	Maya Day	Arbitrary number Vague Year	Day	Goodman pattern number	Maya Long Count Date
1552.540	July 16	197	2288488	1704204	4606.003		#49	11.16.13-16- 4, 12 Kan 1 Pop
13.699			5004		13.709	259		13-16- 4
1539.841	Nov. 3	307	2283484	1699200	4592.296		#35	11.16. 0- 0- 0, 13 Ahau 8 Xul
39.841			14552		39.868	317		2. 0- 7-12
1500.000	Jan. 0	0	2268932	1684648	4552.427		#46	11.13.19-10- 8, 6 Ik 15 Mol
28.408			10376		28.474	156		1. 8-14-16
1471.592	Aug. 4	216	2258556	1674272	4524.000		#19	11.12.10-13-12, 6 Eb 0 Pop
				1651260	=(4524 x 365) =			11. 9. 6-15- 0
				23012	0			3. 3-16-12
				22995	=(63 x 365) =			3. 3-15-15
Remaining count backward				17			# 7	17
to Maya era zero				0				0. 0. 0- 0- 0, 4 Ahau 8 Cumhu

Four dates were calculated for the calibration of Figure 9, accepting the statement that "on November 2, or 3, old style, in 1539 A.D. occurred the end of a katun, which was probably 11.16.0-0-0, 13 Ahau 8 Xul."

The spiral "Z" date line was extended back from the vague year 0 to reach Maya era zero in 23012÷365=63.05 years. The arbitrary year zero being at day 20, the count is 63.01×0.2422 =15.27 days to (20−15.27=) day 4.73=era zero. Note that the 8 Cumhu month position of starting the "Initial Series" day count is day 3 which lacks 1.73 days of agreeing with the above computation of Maya era zero date from Landa's record.

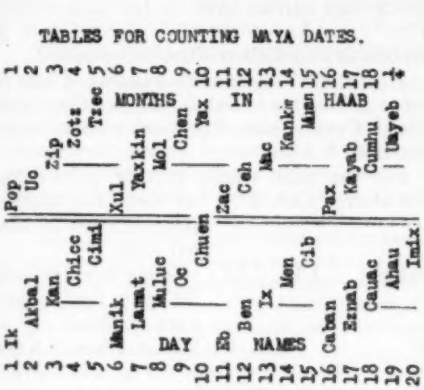
Christian centuries have been laid off from A.D. 1500, as located above, and marked from 1 B.C. to A.D. 16 inclusive, near the center and within semicircles for identification. In traversby the spiral in tropical years 365.2422 days.

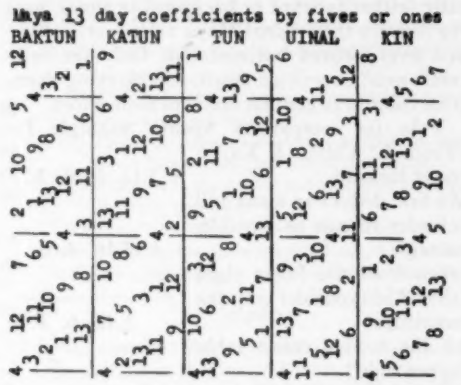
The Christian calendar after 1582 adopted Gregorian years of length 365.2425 days.

With Julian Day numbers and old style dates prior to 1582 we count 365.25 days.

At the 1940 meeting of the Society for American Archaeology in Cincinnati, the writer read a paper entitled "Counting by Fives, Maya Style," in which he contended that only a single standard letter-size sheet of typing is necessary for calculating typical Maya dates. With this, even less work is involved than with Sr. Ola Apenes' table of some 7000 numbers published in 1939 in pages 54–61 of "*Revista Mexicana de Estudios Anthropologicos.*"

Apenes reads off, in turn, the several numbers at particular intersections of vertical columns with corresponding horizontal rows. The alternative here advocated permits a mechanical


TABLES FOR COUNTING MAYA DATES.
MONTHS IN HAAB
Pop Uo Zip Zotz Tzec Xul Yaxkin Mol Chen Yax Zac Ceh Mac Kankin Muan Pax Kayab Cumhu Uayeb
DAY NAMES
Ik Akbal Kan Chicc Cimi Manik Lamat Muluc Oc Chuen Eb Ben Ix Men Cib Caban Eznab Cauac Ahau Imix



Multiples 1-52 of 365 day haabs	(left)	(right)	Maya calendar rounds 1-100	Maya Numbers
1. 0. 5	1.11. 7.15	1. 8.19.17.0	5. 8. 1.11.0	9. 7. 3. 5.0
2. 0.10	1.12. 8. 0	1.11.12.12.0	5.10.14. 6.0	9. 9.16. 0.0
3. 0.15	1.13. 8. 5	1.14. 5. 7.0	5.13. 7. 1.0	9.12. 8.13.0
4. 1. 0	1.14. 8.10	1.16.18. 2.0	5.15.19.14.0	9.15. 1. 8.0
5. 1. 5	1.15. 8.15	1.19.10.15.0	5.18.12. 9.0	9.17.14. 3.0
6. 1.10	1.16. 9. 0	2. 2. 3.10.0	6. 1. 5. 4.0	10. 0. 6.16.0
7. 1.15	1.17. 9. 5	2. 4.16. 5.0	6. 3.17.17.0	10. 2.19.11.0
8. 2. 0	1.18. 9.10	2. 7. 9. 0.0	6. 6.10.12.0	10. 5.12. 6.0
9. 2. 5	1.19. 9.15	2.10. 1.13.0	6. 9. 3. 7.0	10. 8. 5. 1.0
10=10. 2.10	40=2. 0.10. 0	2.12.14. 8.0=20	6.11.16. 2.0=50	10.10.17.14.0= 80
11. 2.15	2. 1.10. 5	2.15. 7. 3.0	6.14. 8.15.0	10.13.10. 9.0
12. 3. 0	2. 2.10.10	2.17.19.16.0	6.17. 1.10.0	10.16. 3. 4.0
13. 3. 5	2. 3.10.15	3. 0.12.11.0	6.19.14. 5.0	10.18.15.17.0
14. 3.10	2. 4.11. 0	3. 3. 5. 6.0	7. 2. 7. 0.0	11. 1. 8.12.0
15. 3.15	2. 5.11. 5	3. 5.18. 1.0	7. 4.19.13,0	11. 4. 1. 7.0
16. 4. 0	2. 6.11.10	3. 8.10.14.0	7. 7.12. 8.0	11. 6.14. 2.0
17. 4. 5	2. 7.11.15	3.11. 3. 9.0	7.10. 5. 3.0	11. 9. 6.15.0
18. 4.10	2. 8.12. 0	3.13.16. 4.0	7.12.17.16.0	11.11.19.10.0
19. 4.15	2. 9.12. 5	3.16. 8.17.0	7.15.10.11.0	11.14.12. 5.0
20=1. 0. 5. 0	50=2.10.12.10 51=2.11.12.15	3.19. 1.12.0=30	7.18. 3. 6.0=60	11.17. 5. 0.0= 90
1. 1. 5. 5	52=2.12.13. 0=1	4. 1.14. 7.0	8. 0.16. 1.0	11.19.17.13.0
1. 2. 5.10	5. 5. 8. 0	4. 4. 7. 2.0	8. 3. 8.14.0	12. 2.10. 8.0
1. 3. 5.15	7.18. 3. 0	4. 6.19.15.0	8. 6. 1. 9.0	12. 5. 3. 3.0
1. 4. 6. 0	10.10.16. 0	4. 9.12.10.0	8. 8.14. 4.0	12. 7.15.16.0
1. 5. 6. 5	13. 3.11. 0	4.12. 5. 5.0	8.11. 6.17.0	12.10. 8.11.0
1. 6. 6.10	15.16. 6. 0	4.14.18. 0.0	8.13.19.12.0	12.13. 1. 6.0
1. 7. 6.15	18. 9. 1. 0	4.17.10.13.0	8.16.12. 7.0	12.15.14. 1.0
1. 8. 7. 0	1. 1. 1.14. 0	5. 0. 3. 8.0	8.19. 5. 2.0	12.18. 6.14.0
1. 9. 7. 5	1. 3.14. 9. 0	5. 2.16. 3.0	9. 1.17.15.0	13. 0.19. 9.0
30=1.10. 7.10	1. 6. 7. 4. 0=10	5. 5. 8.16.0=40	9.14.10.10.0=70	13. 3.12. 4.0=100

tally (either forward or backward in time), using directly the original Maya units (vertically) and fives (spaced horizontally). Only the end result need be written down, and carrying over from column to column involves little effort.

Take for comparison Apenes' example 1: "From 12 Caban 5 Kayab count forward 3.15. 8.10. 3." We first deduct as many full calendar rounds as possible, namely 3.13.16. 4. 0 taken from the lower right table. Then consider only the remainder 1.12. 6. 3 for the tzolkin count table (upper right).

From the initial 12 Caban there is 1 unit to be counted in the katun column. This count is 12-10 the last to be carried over to the tuns column. In the tuns column the count is 12, or 2 fives and 2 ones, thus, 10-3-9 then -5-1. Carry the 1 to the uinals column where the count to be made is 6 (1 five and 1 unit) thus, 1-10 then -4, which last carries over to the kins column. Finally, the kins count of 3 is 4-5-6-7 to 7, the tzolkin day coefficient ultimately reached.

For tzolkin day name the number of kins is 3 forward from the initial Caban, namely, Caban-Eznab-Cauac-Ahau. The final tzolkin date is therefore 7 Ahau.

For the haab count, further deduct from the above 1.12. 6. 3 as many full haabs as possible, from the lower left table,

namely	1.11. 7.15	starting from the initial 5 Kayab, the fractional part of a haab
is	0.16. 8	Distance from 5 Kayab to 0 Pop is 15+20+25
=	2. 0	Leaving, to be counted forward from 0 Pop, 14 months and 8 days
	14. 8	namely to 8 Muan.

The desired date is therefore 7 Ahau 8 Muan.

Grand Rapids, Michigan
October, 1945.

FACTS AND COMMENTS

THE ORGANIZATION OF ANTHROPOLOGY

During the past year there has been much discussion of the need to bring the different branches of anthropology into closer coordination so that they may work together for their common interest. In recognition of this need, the American Anthropological Association at its annual meeting in December, 1945, created a committee which it instructed to

> "ascertain the views of the professional membership of the American Anthropological Association, of allied societies, and of local groups concerning proposals for reorganization of the American Anthropological Association, the establishment of a secretariat, and other means of furthering professional interests."

This Committee represents the different fields of anthropology and consists of J. H. Steward, Chairman, E. D. Chapple, A. I. Hallowell, F. Johnson, G. P. Murdock, W. D. Strong, C. F. Voegelin, S. Washburn, and L. White.

The Committee herewith submits its preliminary findings and earnestly requests that individuals, local groups, and societies give them careful consideration. So far as possible, members of the Committee will endeavor to contact professional persons and groups in order to obtain their reactions first hand. Persons unable to discuss the proposals with Committee members are invited to send their views to the Chairman at the Smithsonian Institution.

The Committee will meet again early in September, and then report its findings and recommendations to the profession. If substantial agreement on the fundamental issues has been reached by the profession, the Committee will submit proposals for appropriate amendments to the constitution of the Association or for other action to the next annual meeting of the Association. It is hoped that any changes can be made effective next year.

Our problem is to devise an organization that can act for the entire profession and at the same time counteract the separatist trends in anthropology as a science. A preliminary count shows that there are some 500 anthropologists in the United States who possess the professional qualifications mentioned below. Not over 250 of these belong to the Council of the American Anthropological Association, though possibly as many as 340 are members of the Association. This means that between 2/5 and 1/3 of the professional anthropologists belong to the Society for American Archaeology, the American Folk-Lore Society, the Linguistic Society of America, the Society of Physical Anthropology, or the Society of Applied Anthropology but not to the Association.

Other professions, which have experienced a similar proliferation of specialized societies, have met the problem in various ways: federation within the parent society; reorganization of the parent society; or creation of a new society. Because the more specialized anthropological societies have a large number of members—some of them, a majority of members—who are not professional anthropologists and because the American Anthropological Association in theory has always represented the entire profession, the most feasible course seems to be to change its structure with the following objectives in view:

(1) Bring into its membership those professionals who are not now members;
(2) Place the control of matters of professional concern in the hands of the professional membership;
(3) Enable it to act quickly and effectively on behalf of the entire profession.

We submit the following recommendations concerning membership, the Executive Board, officers, elections, liaison with other anthropological societies, and a secretariat.

MEMBERSHIP

It is proposed that membership be divided into three classes: Fellows, Members, and Institutional Members.

Fellows.—Fellows shall constitute the Council, which is the professional membership. They alone shall authorize budgets, vote on matters of professional interest, and elect the Executive Board. The Executive Board (below) will carry out their mandates.

Fellows shall meet one of three professional requirements. They must either:

(1) have made significant published contributions in the field of anthropology, or
(2) possess an A.B. degree in anthropology, an M.A. degree in anthropology, or a Ph.D. in an allied field, *and* be actively engaged in anthropology, or
(3) possess a Ph.D. in anthropology

Note: Opinion differs as to how rigid these requirements should be. It has been proposed, for example, that under (3) a person should not only have a Ph.D. in anthropology but be actively engaged in anthropology. On the whole it seemed preferable to start with a fairly broad definition of a professional anthropologist and to allow latitude for interpreting such terms as "significant published contributions" and "engaged in anthropology" so that exceptional cases will not automatically be excluded. If found desirable, requirements may be tightened in the future.

Note: On the belief that many professional anthropologists do not now belong to the Association because they are not interested in taking the *American Anthropologist*, a minority of the Committee recommended that such persons be permitted to become Fellows and have full voting privileges on the Council by payment of small professional dues but without subscribing to the journal. The majority believed that to receive the journal was an essential part of professional membership, but it was recognized that the *American Anthropologist* would have to carry more articles of interest to specialists in the various fields of anthropology.

Members:—Persons who lack professional qualifications shall be eligible to become members. Members shall receive the journal but shall not attend Council meetings.

The Committee was evenly divided as to whether Members should have any voting privileges. The two alternate proposals were:

(1) That Members and Fellows together should elect the

President, Secretary, and Treasurer. This proposal was supported by the contention that non-professional persons would not join the Association if they had no privilege beyond subscribing to the journal.

(2) That all officers be elected by the Fellows, Members being in effect only subscribers and perhaps carrying the designation of "Subscriber" rather than "Member." In support of this proposal, it was held that persons who are not anthropologists would be content to take the journal and present papers at annual meetings, and further, that election of the President by Members would complicate this office (see below).

Institutional Members.—Institutional members shall receive the journal but shall have no voting privileges.

EXECUTIVE BOARD

The Executive Board is conceived as the small, active group charged with the duty of furthering professional interests. Although it will take general directives from the Council and report annually to the Council, it must be empowered to act for the Association at its own discretion between annual meetings as the needs arise. Its membership should include only persons who have expressly stipulated their willingness to devote considerable time to professional matters.

The Committee recommends that the Executive Board shall have 7 members. This is a compromise between a smaller number, which, though more efficient, might represent special interests, and a larger number, which would have greater difficulty in acting. The Board should be elected by the Council, that is, the Fellows, and, so far as practicable, its members should represent the special interests and fields of anthropology. Unless the Board were unworkably large, it would not be feasible to elect its members on a strictly regional or subject matter basis.

Board members should serve for three years and should not be eligible for reelection to the Board for a year after the expiration of their term, but Board membership should be staggered so that two members, in addition to the President or Chairman, are elected annually.

The alternate proposals concerning election of the President also affect the Board. (1) If the Members elect the President, he would be the seventh member of the Board, but the Board would elect its own Chairman, who would be known as Executive Vice President. (2) If the Fellows elect the President, he would also serve as Chairman of the Board. The question is partly whether the Members shall have any voting privileges, partly whether the administrative responsibility and prestige of the presidency shall be separated or combined.

Powers of the Executive Board.—It is recommended that the Executive Board, acting on behalf of the professional membership of the Association, i.e. the Fellows, shall:

Appoint all committees necessary to serve the interests of the Fellows;

Nominate without review all representatives of the Association to the Research Councils;

Appoint all other officers and agents, delegate to them such authority as is required, supervise their activity, and receive and act upon budgets, requests, and plans submitted by them;

Employ and compensate necessary secretarial personnel;

Expend Association funds to meet necessary expenses;

Serve as clearing house for information on the anthropological activities of committees of the Research Councils and on other matters of general interest; and

Publish at suitable intervals a Professional Bulletin, which shall be circulated to all Fellows and which shall report all activities of professional interest.

The Executive Board shall receive its general directives from the Council, made by a majority vote. It shall report annually all activities to the Council and make recommendations for future action. Disapproval of any of its activities and mandates for action different from that taken shall require a two-thirds vote of the Council.

OFFICERS

President, Vice President, Secretary and Treasurer.—Two plans, each favored by half the committee, were proposed for the election of officers.

(1) The President, Secretary, and Treasurer, but not the members of the Executive Board or the Editor, to be elected by the Members and Fellows. This plan includes the further provisions: that the two offices of Vice President, as included in the present constitution, be abolished; that the President be ex-officio member of the Executive Board; and that the Chairman of the Executive Board be elected by the Board members and be known as Executive Vice President.

(2) All officers as well as the members of the Executive Board and the Editor to be elected by the Council. In this case, the President becomes Chairman of the Executive Board and assumes major responsibility for furthering professional interests. The Vice President could be elected by the Board to assume the Presidency in case of need between annual meetings.

The Secretary, Treasurer, and Editor would not be Board members under either plan.

Editor.—It is proposed that the Editor be elected by the Council. If the membership is to be broadened by including professional anthropologists now primarily interested in special fields, the *American Anthropologist* should be broadened so as to cover those fields and at the same time more closely integrated with other journals. To implement this, it is proposed that the editors of the *Journal of Physical Anthropology*, *Journal of Applied Anthropology*, AMERICAN ANTIQUITY, *Journal American Folk-Lore*, *International Journal of American Linguistics*, *Southwestern Journal of Anthropology*, and of other journals to be determined in the future be invited to serve as Associate Editors of the *American Anthropologist*. Closer relations between these editors could well lead to further advantages in lessening publishing costs and wider distribution of the several journals.

In addition to the Associate Editors, the Editor of the *American Anthropologist* should, with the advice and consent of the Executive Board, select such Assistant Editors as are required.

ELECTIONS

Elections should be held at the annual meeting.

It is proposed that in lieu of the present appointive nominating committee, nominations of all officers be made in three ways:

(1) In advance of the annual meeting the Executive Board should nominate three persons for each office (President, Secretary, Treasurer, Editor, and two Board members), with information concerning the candidates, reasons for their choice, and in the case of Board members, assurance of the nominees' willingness to devote considerable time to Board work. These nominations should be published in a professional Bulletin two months before the annual meeting.

(2) Additional candidates may be included in the slate at the request of five Fellows, or

(3) Nominated at the annual meeting, provided each nomination receives four seconds.

It is also proposed that elections shall be by secret ballots which may be cast in three ways:

(1) In person at the election,

(2) By mail, provided the ballot has been received by the time of the election meeting,

(3) By proxy.

It is proposed that Fellows attending the annual meeting may vote in elections and other business for not more than two proxies. This is the customary limitation to prevent any person from controlling a large number of votes.

LIAISON

As any proposal to meet professional needs entirely within the framework of the American Anthropological Association cannot take the other five anthropological societies formally into account, it is proposed that each of these societies be invited to designate a liaison officer to the Association. These officers would be empowered to recommend action to the Executive Board and they could be invited by the Board to advise on matters relative to the fields of anthropology they represent. They could not, however, have official membership or voting privileges on the Board.

SECRETARIAT

There has been much discussion of the establishment of a permanent Secretariat to serve the professional interests of anthropologists. The Committee believes that the establishment of such a Secretariat is one of the various steps which the Executive Board might take on behalf of the profession, but that a Secretariat and its financing are quite unrelated to the immediate problem of organization. Many of the immediate needs could be met through limited projects, each financed independently. The financing of a permanent Secretariat is a problem that need not be faced at present and that should not prejudice the proposals concerning organization.

TO FACILITATE COOPERATION IN THE IDENTIFICATION OF MAMMAL BONES FROM ARCHAEOLOGICAL SITES[1]

I have just completed the examination of about 20,000 fragments of mammal bones comprising seven archaeological collections. One collection contained 13,293 fragments, another, 2745 and a third, 2109. The localities represented ranged from Coahuila, Mexico, to western Pennsylvania, and the greater part of six months was required to make the identifications.

Such work, involving great numbers of specimens from many localities and a wide variety of local habitats, constitutes a problem for both the zoologist and the archaeologist. It is also a special problem for institutions which consent to identify such collections. To facilitate cooperation in such identifications, I offer the following suggestions to the archaeologist.

1. A different field number should be assigned to each lot of associated bones. Individual bones need not be numbered, but lots should be labeled as carefully as artifacts. Changes in fauna through the disappearance or introduction of species by natural or human means can then be checked. This is an important matter to both the archaeologist and the zoologist, and levels should be indicated clearly.

2. A 4″×6″ card, labeled with the field number, should be prepared for each lot of bones. This enables the examiner to write the identifications on the cards, and to return them without typing long lists.

3. All bones should be packed carefully to prevent breakage in shipment. Associated parts should be glued or tied together in the field when the association is plainly evident. Complete bones can be identified more readily than fragments. Care should be taken also to prevent bones from becoming separated from their proper lots and mixed with loose bones from other lots. When this occurs they never can be restored to their correct associations.

4. All bone specimens should be cleaned of adhering soil and incrustations so that the opening of a container does not spread dirt and dust over the desk and room, and so that the identifier does not have to take additional time to clean the specimens. When dirty bones are numerous in a collection, there is a tendency to discard them as unidentifiable.

5. As a courtesy to the identifier, who may spend weeks or months studying the material, the archaeologist should invite him to prepare a zoological account either as an appendix to the archaeological report or for separate publication in a zoological journal. The zoologist can supply the zoological data, while the archaeologist can discuss the anthropological details (ethnozoology); duplication can be avoided. The zoological details are sometimes of extreme interest and importance, as, for instance, extensions of range in the past, change of fauna, and distinguishing skeletal characters not noted hitherto. These data should be made available to zoologists.

[1] Published by permission of the Secretary of the Smithsonian Institution.

6. A description of the ecology of the site or region should accompany the collection. Plains, chaparral, hardwood forests, coniferous forests, caves, deserts, and river courses have characteristic mammalian faunas. With ecologic information, the identifier is in a better position to estimate the total fauna to be expected, and to evaluate the excavated material. In addition, it will be easier to judge any different conditions in former times which may be indicated by the material.

7. Local state museums or universities should be encouraged to identify the bone fragments. Bones that cannot be identified locally because of inadequate comparative material may be sent to a specialist in a larger institution. At the local institution it should be possible to determine accurately, or to discard as unidentifiable, at least 75 to 90 per cent of the fragments. Furthermore, it is suggested that institutions making the identifications be permitted to retain material which they desire. Bones representing dog, or species extinct today, or those which establish records of former range extension, should be saved in some institution where they can be re-examined at any later time. In the case of large collections, it is desirable to obtain the willing cooperation of an identifier beforehand, and not to overwhelm him with several months' work without warning. The identifier should anticipate the task. Lastly, when one collaborator obviously is not interested in continuing such work, arrangements should be made elsewhere. However, the time may come when there will be certain specialists in bone identification who will welcome this type of work.

8. Only in exceptional cases can an archaeologist become sufficiently adept in comparative osteology to make all the identifications himself. However, he can prepare or obtain from competent authorities lists of the vertebrates (and molluscs) of the area to be excavated. In the course of such archaeological excavations, it may be possible for the archaeologist to make notes on the relative abundance and possible ethnozoologic significance of the living species observed during the period of his work. The archaeologist thereby would appreciate more thoroughly the significance of the animal remains which are dug from the caves, pits, or trenches. It is suggested that he ascertain from the staff of the local institution what critical material from the living fauna they wish to have collected for making their identification comparisons and that he obtain it if feasible. The preparation of a skeleton in the field is not a difficult procedure. The skin, entrails, and large muscle masses are removed. The head (with hyoid or tongue bones attached) and the tail are disjointed. Measurements are made before skinning on (1) total length (nose to last tail vertebra), (2) hind foot (heel to tip of longest claw), and (3) ear (from notch); after skinning on (4) tail (base to tip), and (5) trunk (atlas to rear of sacrum). The carcass, with tail and head (brains removed with stick and water), is tied together, allowed to dry in almost normal shape, and has affixed to it a large, durable tag giving collector's name, collector's number, locality, date, sex, and measurements as listed above. The number and data should be duplicated in the field notebook. Weight is desirable if it is obtainable. On large archaeological projects, where a number of people are involved over a period of years and the yield of animal remains is considerable, it might be well to include a vertebrate zoologist or ethnozoologist as an interim member of the expedition.

All this, I believe, would lead to a more satisfactory cooperation between collaborators, and such collaboration between the different fields of science is more necessary with each passing day.

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A RECONSIDERATION OF SHELLMOUNDS WITH RESPECT TO POPULATION AND NUTRITION

A good many years ago Nelson[1] and Gifford[2] made a careful and extensive survey of the shellmounds of San Francisco Bay, in the course of which they utilized quantitative methods of analysis to determine the probable age of the cultures represented. In the years since their publications, new deposits have come to light in various parts of the Americas to which their methods might be applied, and a great deal more knowledge is now available which is applicable to the field of primitive dietetics.

There are two possible methods of attacking the problem of the relationships of quantity of shell to population and age—volume and weight. Nelson used the first, Gifford the second.

Nelson calculated the volume of the mound at Ellis Landing, near Richmond, California, as 1,260,000 cubic feet. He then assumed that the mean daily consumption per person was fifty mollusks, in this case the Bay mussel, *Mytilus edulis*. From house pits and other evidence the population was placed at 100. Hence the total daily consumption was 5000 animals. He then proceeds: "Actual trial shows that the volume yielded by the total 5000 shells, crushed down to their present consistency would be about 1200 cubic inches. To this amount should be added a quantity of ashes, broken rock and such extra debris as may collect about a camp—possibly sufficient to make up one cubic foot for the daily average. Calculated on this basis it would apparently have required about 3500 years to accumulate the pile."[3]

The methodological difficulty lies in Nelson's value for the volume of 5000 crushed shells. He evidently thought in terms of the pieces having been crushed; embedded in a matrix of ash, refuse, and soil; and packed in by years of pressure so that the interstices were filled with air or soil water, as the case might be. But the overall volume of pure crushed shell in the laboratory is not the same as under field conditions. Moreover, it is difficult to see how he derived the value of 1200 cubic inches. In order to make a check, the present writer secured forty fresh specimens of *Mytilus edulis* from San Francisco Bay. The shells were cleaned of

[1] Nelson, 1909, 1910.
[2] Gifford, 1916.
[3] Nelson, 1909, p. 346.

meat, freed of organic matter by treatment for twenty-four hours with 20 per cent NaOH at 60° C., and dried in an oven. The volume was determined by direct water displacement. The mean volume per shell (both valves) was 0.948 cc. That of 5000 shells is therefore 4740 cc., which is 289 cubic inches. Nelson assumes a ratio of shell to matrix of 1200 to 1728. Applying this ratio with 289 cubic inches and retaining Nelson's other constants we get an age for the mound of 14,500 years.

Since Nelson apparently used, not the actual volume of 5000 shells, but the space occupied by them in the crushed state, including air, it might be possible to utilize specific gravity. Gifford found that the specific gravity of mound material was 1.3. The determinations in the laboratory for both mussel and clam yield a value of 2.7 for shell itself. Since Gifford found that the content of the Ellis Landing mound was 69.43 per cent shell, the volume of the latter would be 9500 cubic centimeters and the number of shells 10,000. Assuming Nelson's rate of consumption the age would be calculated at 6900 years.

It is clear that the attempt to utilize volume, either of the actual shell substance or of the space it might occupy in the ground, is beset with difficulties owing to uncontrollable variables, which render a reasonable result highly uncertain. A simpler, more consistent approach is through measurements of weight.

Gifford[4] determined the quantity of shell by screening the mound material through meshwork of different sizes, and by chemical analysis. His data appear highly reliable and may be accepted without qualification. From the known dimensions of the mound and its mean specific gravity he estimated its total weight as 51,085 short tons. The shell comprised 69.43 per cent of the total by weight: 35,468 tons, or 32,169,476 kilos. However, he then arrived at the rate of deposition from Nelson's time estimate of 3500 years and concluded that it was fifty-six pounds (25.4 kg.) per day.[5] He adds that "this amount of shell seems reasonable enough, if we accept one hundred people as the average population of the mound throughout its growth." Nevertheless, some further analysis of the situation is desirable.

In his table 13,[6] Gifford shows that of the total shell at Ellis Landing approximately 62 per cent was *Mytilus* and 38 per cent *Macoma nasuta* (clam). Therefore the former contributed 19,945,075 kg. and the latter 12,224,401 kg. All of Nelson's calculations were predicated upon consumption of mussels, but obviously the consumption of clams must also be considered. It has been suggested that twenty clams be regarded as equivalent to fifty mussels, but this seems to me too high. The gathering of clams, which must be dug individually, involves much greater labor and time than does the gathering of mussels, which may be scraped in masses off the rocks. Moreover, experiments performed in the laboratory show that the meat of a clam weighs on the average about eight times as much as that of a mussel. Hence, to obtain the equivalent in food value of fifty mussels only six clams would be needed. In the course of the same experiments, however, it was found that the mean weight of a clean, dry mussel shell is 2.5 gm.; that of a clam shell (both valves), 29.75 gm. Therefore, by Gifford's weight data and direct division the mound contained the remains of 7,879,000,000 mussels and 410,942,000 clams. This is in a ratio of 2.57 clams per fifty mussels, somewhat less than the estimate based on diet.

If we now use Nelson's assumption that the inhabitants of Ellis Landing consumed fifty mussels per person per day (neglecting clams) and that the mean population was 100 persons, then the age of the mound is 7,879,000,000 ÷ (5000 × 365), or 4370 years. When clams are included, it becomes necessary to use weight rather than number of shells. Since Nelson's figure of fifty shellfish gathered per person per day is a pure guess, it will not seriously modify his statement to presume that in addition to fifty mussels each person gathered 2.57 clams. Then the weight of shell gathered per person per day is 125 gm. for mussels and 76.5 gm. for clams, a total of 201.5 gm. This means 7,342,750 gm. per year for the village. The age of the mound then becomes 4380 years.

In recent years we have learned that throughout prehistoric time the coast natives exported large amounts of shellfish to the interior, in particular to the lower San Joaquin and Sacramento valleys. Some of the mollusks were shipped in the shell, but the far more economical and efficient method was to export only the dried meats. The exact extent of this trade is unknown, but certain facts are pertinent. The mean wet weight of the meat of one mussel, as determined recently from fresh material, is 1.065 gm. The dry weight is, like that of most soft tissue, about 20 per cent of the wet weight, or about 0.2 gm. Then one kilogram of dried meats would represent 5000 animals and, in terms of Nelson's assumption, would correspond to one day's labor on the part of the entire village population. One hundred kilos of dried mussels a year for export does not seem excessive, since this would require only one trip annually to the interior for four men, each carrying 25 kilos. If export was of this order of magnitude, then actually from one-quarter to one-third of the shellmound, say 30 per cent, represents export. On the other hand, there is no reason to modify Nelson's original idea that 5000 mussels, plus 200–300 clams, were gathered daily for home consumption. If such was the case, the gross rate of gathering was a good deal greater than Nelson thought. How much his value should be increased is a matter of conjecture.

If 30 per cent of the deposit represented export, then the gathering rate must be set at seventy-two mussels per person per day and the age becomes 3070 years. If it can be conceived that 50 per cent was export, then the age would be 2190 years. To assume higher values and still retain Nelson's rate of home consumption would, it appears to me, call for improbably great effectiveness and industry in the actual gathering. To reduce the basic rate of consumpion—fifty mussels per person per day—would mean again extending the age of the mound. With regard to age, it must be admitted that this re-examination of the data, even allowing for commerce in shellfish, cannot permit a duration of less than 2000 years. More satisfactory is at

[4] Gifford, 1916. [5] *Ibid.*, p. 12. [6] *Ibid.*, p. 27.

least 3000, thus in general confirming the original supposition of Nelson and Gifford.

For purposes of discussion, let us assume a home consumption of fifty mussels and 2.5 clams per person per day. In terms of meat this is 53+21, or 74 grams. The chief dietary value of such food is in the animal protein and, hence, essential amino acids thereby furnished, since the vitamin content is negligible. According to Rose's Table I, 100 grams of clams furnish about 7.5 grams of protein.[7] Presumably the protein content of mussels is of the same order. Therefore, the aboriginal daily intake would have been about 5.5 grams. This is far below the minimal maintenance intake of 25 to 30 grams. But it must be remembered that the natives were eating many types of food, particularly plant material, which furnished considerable protein. The latter may have been of low biological value, although adequate in quantity. It has been repeatedly shown that under such circumstances the addition of very small amounts of high-value animal protein is sufficient to supplement the low-value protein and produce an adequate diet. There can be no doubt that in this respect the presence of shellfish was of major importance.

As a more or less speculative check upon the validity of the procedures and calculations developed for the California shell mounds, it is of interest to attempt their application to an entirely different geographical and cultural background. If the results were in general conformity with those obtained by quite different methods, then there would be justification for pursuing an intensive investigation and analysis at the site in the hope of achieving a fairly high degree of precision with respect to population, age, and dietary conditions.

A possible trial area is one which is very briefly mentioned by Ekholm in his monograph on the Huasteca.[8] On page 421, he refers to the Isla de Pithaya, which he says is an island in a lagoon near Tampico, Mexico, some 100 yards offshore. This island has an approximate area of 100 acres and stands 6 meters above the water level. Cultural deposits "apparently make up the bulk of the island" and include "a great amount of shell." These deposits are of such mass and density that "much of the island" has already been dug up by the local people to burn for lime. With no further information than is thus sketchily conveyed in published form by Ekholm, what can be accomplished by application of methods applied to the California sites?

The area is 100 acres or 405,000 square meters. The maximum height is six meters. If the form is that of a cylinder with vertical sides the volume is 2,430,000 cubic meters, but if the form is lenticular the volume is roughly one half that value, or 1,215,000 cubic meters. The latter form is more probable and may be assumed until actual measurements are made.

The volume is about eighty times that of Ellis Landing and the area nine times as great. It will not be unreasonable, therefore, to estimate the mean population at 1000. Then, if we apply directly Nelson's deposition rate, 10 cubic feet per day for 1000 persons, we get an age to the Conquest, or A.D. 1520, of 11,700 years, a quite absurd result.

We may, however, employ the weight method. In analyses on fifteen California mounds, Gifford found a mean shell content by weight of slightly over fifty per cent.[9] In view of the obviously high shell density of Isla de Pithaya, this seems a fair value. Consequently, if the specific gravity is 1.3, the total mass of material is 1,458,000,000 kg. and that of the shell alone is 729,000,000,000 grams. Ekholm does not specify the type of shell, but in connection with similar deposits at other sites he mentions clams and oysters. Apparently mussels were not present. In the absence of any direct evidence, let us assume an approximate equality in the weight of shell of these two types of mollusk, or a total for each of 364,500,000,000 grams. Since the species are likewise unknown to me, there is no alternative but to apply the mean shell weights found by direct measurement on samples of Pacific Coast clams and oysters. These are 29.75 gm. for clams, and 116.7 gm. for oysters; therefore the total numbers are respectively 12,250,000,000 and 3,120,000,000.

With regard to rate of gathering, Nelson's assumption of fifty mussels per person per day is inapplicable. On the other hand, the individual industry and labor which could produce fifty mussels plus two or three clams on San Francisco Bay, if applied entirely to clams and oysters, should be able to account for, let us say, twenty clams and five oysters on the lagoons of Tampico. Using the clam component, we get 7,300,000 per year and the age of the deposit turns out to be 1750 years. Using oysters as a basis, the age is 1710 years.

There was undoubtedly commerce involving shellfish. The interior of central Mexico has been for centuries heavily populated, and there is much evidence that the tribes in the latter region were well acquainted with both Atlantic and Pacific shellfish. Moreover, certainly in late Aztec times molluscan food was exported from the coasts. A purely arbitrary factor to account for this trade would be 20 per cent. Then, if home consumption is not affected, the age must be reduced to about 1400 years.

These calculations must be regarded as purely tentative, subject to revision when and if the actual field data concerning the shell deposits become known. But it can scarcely be due merely to chance that the estimate based on shellfish—1400–1800 years—comes quite close to that of Ekholm based on ceramic sequences in the Panuco-Tampico region—1000–1500 years.

With respect to the nutritional status of the inhabitants, it can be shown that twenty clams and five oysters will supply an average daily intake of about 16 grams of protein. As suggested previously, this constitutes an invaluable supplement of essential amino acids and vastly improves the diet of a primitive people subsisting primarily on plant sources. If the export of this food to the interior can be demonstrated to have occurred on a substantial scale, it will be of considerable significance for the human ecology of aboriginal central Mexico.

[7] Rose, 1938.

[8] Eckholm, 1944.

[9] Gifford, 1916, p. 15.

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A COMMENT ON "THE PEDESTAL VESSELS OF THE MADISONVILLE SITE"

In a recent note,[1] John W. Griffin has suggested that the form of the pedestal vessels from the Madisonville site is derived from a European model. He quotes Holmes, who thought the design was possibly copied from the work of southern neighbors (Middle Mississippian?), but rejects this conclusion on the basis that "the pedestal of this vessel can hardly be considered related to the annular bases and occasional pedestal-like bases of Middle Mississippi vessels."

At least one vessel very similar in form to the Madisonville specimens has been obtained at a Middle Mississippi site. It is figured by Conant,[2] and its provenience is reported as New Madrid, Missouri, but unfortunately we do not have a description of the piece. The illustration shows a shallow bowl on a pedestal with an expanded base. The surface is smooth and there are no handles. No scale accompanies the figure, so the exact size is not known. The pedestal accounts for about one-half the height of the vessel and at the point of its greatest constriction it has a diameter about one-third that of the mouth of the vessel. Whether the pedestal is solid, as in the Madisonville specimens or hollow, and, if the latter, whether or not it is connected with the vessel proper cannot be ascertained.

In specific features, the New Madrid specimen differs from those from Madisonville. It is a bowl, the latter are jars; it is smooth, the Madisonville specimens are cord-marked; and it lacks handles, which are present on one of the specimens from Madisonville. In spite of these differences, the impression gained from a comparison of Conant's illustration with James B. Griffin's illustration of one of the Madisonville specimens[3] is that the forms are so closely related that one could be derived from the other or that they might owe their inspiration to a common source. Holmes may have had this New Madrid specimen or a similar one in mind when he suggested a southern origin for the form.

The Conant vessel can be attributed to the Middle Mississippi phase with a high degree of probability. His illustrations of specimens, taken in toto, represent a characteristic assemblage of the artifacts associated with Middle Mississippi sites in southeastern Missouri and northeastern Arkansas (effigy and painted pottery, biconcave discoidals, chipped and polished flint celts, and incised shell gorgets). His site descriptions strengthen this conclusion (rectangular, flat-topped mounds; embankment enclosures; house pits; extended burials with pottery vessles; etc.). The Middle Mississippi manifestation in this area has been called "St. Francis." Museum specimens derived from it are extremely common throughout the country. In spite of this fact, we not only have no comprehensive description of the culture, we do not have even a single site described in modern scientific style. The reports of Potter and Evers, Thomas Beckwith, Clarence B. Moore, and the shorter works of several other authors indicate a good deal of heterogeneity in this culture. This heterogeneity probably reflects temporal differences, but only the most general statements about time are possible. Developed Middle Mississippi sites are late in the stratigraphic column wherever they occur, but no specific contact trade objects have been reported in the meager literature available on St. Francis sites.

James B. Griffin has strongly stressed the derivation of certain specific pottery traits of the Fort Ancient aspect (e.g. effigy vessels and salt pans) from the Middle Mississippi phase.[4] It seems unnecessary to postulate a European form as the inspiration for the pedestal-based vessels from Madisonville, since there exists at least one aboriginal model in a culture from which Fort Ancient had borrowed.

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[1] John W. Griffin, "The Pedestal Vessels of the Madisonville Site," AMERICAN ANTIQUITY, Vol. 10, No. 4 (1945), p. 386.

[2] A. J. Conant, *Footprints of Vanished Races*, St. Louis, 1879, p. 33 (third row from top, second figure from right).

[3] James B. Griffin, *The Fort Ancient Aspect*, University of Michigan, 1943, Pl. 66, Fig. 4.

[4] James B. Griffin, *op. cit.*, p. 260.

THE ATLANTEAN COLUMNS AND THE LINTEL OF THE INITIAL SERIES TEMPLE AT CHICHEN ITZA

A portal with foreign ornamentation is more complex than a plain, unadorned doorway. Mexican by derivation, the Atlantean columns of the Initial Series Temple at Chichen Itza were incorporated in Maya architecture. The sculptured figures were given uplifted arms in a pleasing conceit that strong arms sustained the overhead load. These uplifted arms caused projections alongside the pair of heads, so that a stone lintel which rested solely on the inner projections would have had insecure support; hence the requirement for greater extension of the lintel on the Atlantean columns. Some marginal covering of the inscription on the underside of the lintel apparently resulted, but it did not affect the initial series, 10.2.9.1.9, 9 Muluc 7 Zac. Fully displayed on the front of the lintel was the paramount

dedicatory date of the Initial Series Temple, 10 Tun of Katun 1 Ahau.

In the construction of the imposing entrance to the Temple, the Atlantean columns of Mexican origin and the stone lintel with Maya inscriptions were fittingly combined by the artisans of Mexican-ruled Chichen Itza.

It is certain that a lintel rested originally on top of the Atlantean figures which flanked the front doorway of the Temple. A tangible stone lintel was discovered on the ground near the basal drums of the Atlantean figures where a stone lintel, dislodged from the tops of the supporting columns by the growth of big trees, would have fallen. On front of the lintel is recorded a dedicatory date that corresponds to 10.2.10.0.0. The conflict of this date with some preconceptions may have provided an incentive to doubt that the original position of the lintel was over the main entrance to the Temple.

A suggestion that the Initial Series lintel was re-used as a wall stone is fanciful, in as much as no concrete evidence has been presented to show that the lintel was a wall stone. The deposit of a "wall stone," with the characteristics of an inscribed stone lintel, adjacent to the basal drums of columns that had supported a lintel is too coincidental for credence.

In *People of the Serpent*, 1932, the discoverer of the Initial Series lintel, Edward H. Thompson, expressed the commonsense view that the inscribed stone lintel had rested on top of the Atlantean figures until dislodged by the growth of big trees.

Inscribed stone lintels, before re-use in native architecture at Chichen Itza, may have been shattered to permit escape of tutelar spirits; a practice which would account for failure to find there a stone lintel bearing a dedicatory date prior to the Maya-Mexican era.

The Initial Series lintel is a carved monolithic tablet with hieroglyphs in good condition. According to the weight of factual evidence, this dedicatory lintel formed an original component of the main entrance to the Initial Series Temple of Maya-Mexican construction at Chichen Itza.

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CONCERNING GROOVED AXES IN CALIFORNIA

The distribution of grooved axes in California as given by Heizer[1] is complete with only the following minor exceptions. On display in the Chicago Museum of Natural History there are two full-grooved axes labelled "Hupa" and a third attributed to the Achomawi.

While along the Pacific coast weights and hammers were grooved, there are only a few grooved axes. These are of three types.

1. Three-quarter- or full-grooved axes—as indicated by Heizer—found commonly in the Southwest.
2. The partially grooved adz, found in British Columbia and Alaska.
3. A peculiar type of "wedge or chisel," essentially an elongate, widely-grooved ax, reported by Strong, Schenck, and Steward from the Dalles-Deschutes region.

It is obvious that some factors inhibiting the adoption of the grooved ax were operative along the entire Pacific coast from Alaska to southernmost California, and were especially powerful in California, where even celts are scarce.

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[1] Robert F. Heizer, "The Occurrence and Significance of Southwestern Grooved Axes in California," AMERICAN ANTIQUITY, Vol. 11, pp. 187–193.

BOOK REVIEWS

The Adena People. WILLIAM S. WEBB and CHARLES E. SNOW. (University of Kentucky, Reports in Anthropology and Archaeology, Vol. VI, 369 pp., 28 figs., 7 tables, 1 map, University of Kentucky, Lexington, 1945.)

This volume is presented as a summary of the Adena trait complex in Kentucky. Since 1932, when Greenman's "Analysis of the Adena Culture" appeared, a number of additional Adena sites have been explored in Kentucky. These have furnished information which makes possible a more complete description of the culture and throws new light upon its affiliations. To Greenman's list of seventy mounds, the authors have added twenty new sites for Kentucky and eighty-three for the whole Adena area. To his list of fifty-nine traits, they have added one hundred fifty-nine newly delineated traits. All traits are presented in a well-arranged list which gives Greenman's original numbers and the sites in which the traits have been recognized. In one chapter, each trait is described and discussed in considerable detail. This section is a very valuable portion of the volume which other archaeologists would do well to study. The style of presentation would reach its peak with the inclusion of photographs of each material trait.

Although the reviewer fully appreciates the value of this new study of the Adena culture, he considers it his function to point out factual errors, fallacies in method, and what he considers unproven theories. For example, the authors state (p. 31): "It has recently been demonstrated that 'sacred enclosures' or sacre [*sic*] circles are of Adena origin." In support of this statement they refer to page 167 of Volume V, No. 2, of the Kentucky reports dealing with the excavation of Unit A of the Mt. Horeb Earthworks. There Webb states, "From the evidence at hand, although meager, one must conclude that this 'sacred circle' was the work of the Adena people. The circular pattern of paired postholes seems diagnostic of that Complex." Then, on page 33 of

the volume being reviewed, the authors express their belief that "*every 'sacred circle' is of Adena origin*, and that many of them still present internal evidence of that origin. Careful excavation of the central enclosed area should reveal a *circular pattern of paired post-molds*." Thus, from one case admittedly based on meager evidence, they jump to the conclusion that all such circular earthworks are Adena and have paired post-molds. Furthermore, for those words here printed in italics, they use bold-face type to emphasize their point! Their statement lacks proof and is definitely misleading.

The Wilmington Mound (pp. 81–133) is not in the list of mounds presented by the authors, nor is it No. 126 of their list. The Wilmington Tablet was found in the Sparks Mound, not in Greenman's Mound 25. Mound 4 of Greenman's list is not identical with Mound 5 of the Hopewell group, as stated by the authors (p. 132). Furthermore, the error is expanded when they state, "This seeming error illustrates what is sought to be emphasized in this study, that is, the very great similarity between Adena and Hopewell burial sites frequently found."

Trait 153, "representation of the serpent motif," should probably be omitted from the list. The authors accept Willoughby's interpretation of the Cincinnati Tablet motif as that of a serpent, while in the reviewer's opinion it represents the raptorial bird which occurs on most of the other tablets. It is probable, too, that the design of the Gaitskill stone tablet represents a bird rather than a serpent. Finally, evidence presented by Griffin does not definitely establish Serpent Mound as Adena; it may be Hopewell. Strong doubt may be cast on the validity of trait 154, "human facial mask (death motif?)." It is said to occur in the Gaitskill stone tablet design, along with a serpent motif. This interpretation cannot be justified. The hand-eye design, trait 155, is stated to occur on the Gaitskill clay tablet. To the reviewer, the design referred to is a part of a bird motif, as on some other Adena tablets.

The authors present a map of the "Adena Occupation in the Ohio River Basin" on which 169 mounds classed as Adena have been plotted (p. 132). On it the six major Ohio Hopewell sites are also marked in order to show that "the smaller Ohio Hopewell area is almost exactly centered in the larger Adena area." They argue that this implies a fundamental connection between the two cultures. However, there are many other Hopewell sites in Ohio with a much wider distribution than that implied by the authors; furthermore, the fact that some Hopewell sites occupy the same area as Adena sites does not mean that Adena is ancestral to Hopewell. In addition, all non-Ohio Hopewell sites are left out of the discussion by Webb and Snow.

The following chapter is entitled "The Relation of Adena to other Hopewellian Manifestations," although the discussion is restricted to Ohio Hopewell. The authors believe that the common people were cremated and the selected few were inhumed. "High development of culture would therefore be measured by the skill and artistic achievement manifested in the artifacts found in inhumations, rather than by the practice of cremation. On such a basis Tremper cannot be assigned to the top of the cultural ladder—but must be put at the foot of the ladder, the lowest cultural level of Ohio Hopewell, for reasons to be presented later." In making such a statement the authors have either forgotten or ignored the great cache of effigy platform pipes from Tremper which equal in artistic conception any other type of Hopewell artifact. Do they believe that these and other fine artifacts from Tremper were buried with common people?

The next chapter includes a "Table of Comparative Burial Traits" in which an attempt is made to show "that by comparing burial traits only, there is shown for Adena a closer relationship to Ohio Hopewell than it is possible to show for the 'Trempealeau Culture,' which has been designated Elemental or Basic Hopewell." The table does not show this to be true, for, as the authors state, "the absolute value of the percentage correlation may mean little or nothing since the trait list must be considered to be incomplete and by slightly changing the statement of the traits, a different numerical result could be obtained." They believe Trempealeau was contemporaneous with Late Hopewell in Ohio.

Under "Widespread Hopewellian Manifestations," there is a trait list which compares Adena, Hopewell, Copena, and Trempealeau, although no specific conclusions are drawn from it. The incompleteness of the treatment is attributed to nonavailability of necessary information.

The authors then take up what they consider the "Evidence of Gradual Development of Traits in Hopewell." They hold that "the distribution of copper earspools among Hopewell sites is very non-uniform, indicating a definite developmental trend." A nonuniform distribution does not necessarily indicate a definite developmental trend. The scarcity of certain artifact types at particular sites may be explained on several grounds; for example, general scarcity, some objects not placed commonly in mounds, specimens in the hands of a selected few, or specimens destroyed by cremation. They regard Tremper as the earliest of the Ohio Hopewell sites and Hopewell as one of the last sites to be abandoned. Hopewell had 131 earspools and Tremper eight. "Further, of the eight copper earspools actually found at Tremper some were of a cruder form (one side being flat instead of concave). Here is evidence that the copper earspool did not appear suddenly but developed not only from a crude form at Tremper to a finished form at Hopewell, but in its use from two per 100 burials to 166 per 100 burials." If the authors had examined the copper earspools from Tremper, they would never have made such a statement. The single pair of earspools with one flat side are not crude, but are as finely made as the ones from the Hopewell group. Furthermore, two other finely made earspools of the type found at Hopewell were taken from the same grave as the so-called "crude" pair.

Webb and Snow believe that the platform pipe in Ohio Hopewell developed from an early Algonquian type, and that "from this simple form Hopewell may have developed the straight-based monitor pipe, then the curved-based monitor pipe, and finally, the curved-based effigy pipe of late Hopewell, the highest development." Tremper and Mound City had more curved-based effigy pipes than any

of the other sites, a fact which, on the basis of the above statement, would place them as late Hopewell! Again the authors have ignored the great cache of pipes from Tremper. Their conclusion that "in the majority of the important Hopewellian traits one is able to see evidence of gradual transition of Adena into Hopewell" is not established by the evidence presented.

The next section compares Hopewell and Adena burial traits, starting with the assumption that burial traits are more important than artifact traits. "Burial practices and the resultant traits which point to them may thus be conceived as the social act of the body politic, in contrast to, for example, the manufacture of a particular type of artifact which may be considered the act of an individual, in which society generally had no interest, and on which it exerted little or no control." On the contrary, there is good evidence that artifact types in preliterate groups are strongly bound by tradition. The assumption is apparently forced because Adena burial traits seem closer to Ohio Hopewell than do Adena artifact traits.

The writers selected the Hopewell mound group for their detailed comparison with Adena burial traits, pointing out the close similarities in the burial customs of the two peoples. First defining cremation as the burning of a body in the flesh, they state that such cremations did not take place at Hopewell, because the crematory basins were too small for that purpose. Hence, "these authors prefer to believe that these basins were 'fire basins' placed on a house floor, probably near its center, but where the house was large, and more than one basin was built, they may have been nonsymmetrically placed. . . . If this long discussion has lowered the exalted status of these clay basins from *sacrificial altars* to *crematory basins*, and finally to *simple fire places* [Again, the authors have used bold-face type in place of the italic type used here.] used in the domestic life of the household, what is the effect of this change of status on the understanding of the burial customs at Hopewell Group of mounds?" Continuing, they state that "these cremations of bones, not bodies, might perhaps have been performed outside of the house in the village nearby, but they could have been done on the very small fire places on these house floors." The reader is led to believe that the basins and other features found at Hopewell are the same as those at the other Hopewell sites. This is not the case. For example, large crematory basins occurred at Mound City and Tremper. It is hard to envision these as "simple fire places." A careful study of *all* the major Hopewell sites indicates that the burials and offerings were made on the floors of house structures, as stated by the authors, but that these house structures were not ordinary dwellings. It is probable that they were ceremonial houses and/or houses of the dead which were used for a considerable period of time for burial purposes before they were burned and covered with primary mounds. The elaborate graves, the great offerings, and the complicated earthworks all point to special ceremonial and perhaps ruling centers where only the more important personages of the group were buried.

The authors believe that Tremper "closed so early that most of the indicators of high cultural development had not been developed at the time of its discontinuance. It had no copper breastplates, no copper headplates, no metal covered buttons, no cut bear teeth, no silver, and no obsidian." This is not in accord with the facts, for Tremper did have metal-covered buttons and cut bear teeth. Continuing, they hold that the "Tremper site may have been so early that it was abandoned before they had adopted log tomb burial for persons held in high esteem, and so all were cremated." If Hopewell developed from Adena, and Tremper is early Hopewell, it is hard to follow this line of reasoning. To explain the same lack of log tombs at Mound City, they state the site was so thoroughly destroyed before final investigation that one is not sure that they did not exist. The site was not thoroughly destroyed before exploration and it is almost certain that log tombs did not occur.

"Relative Chronology in Ohio Hopewell" is taken up in the next chapter, with the authors maintaining that, if Ohio Hopewell developed out of Adena, then one should be able to trace developmental stages in Ohio Hopewell. They assume that sites showing the highest development either persisted for a long time or else are late sites. They further assume that the material culture of the whole group was homogeneous at any one time. Then, "Since most of these artifacts are assumed to occur definitely as burial offerings, the total number of artifacts from any site, as a first approximation should be proportional to the number of burials discovered at that site. If, therefore, the number of artifacts of a given kind per 100 burials could be determined, such ratios would constitute coefficients of occurrence density, which would permit a more exact comparison of two sites of different sizes, on a roughly quantitative basis." They believe that "that site is the latest which has the largest number of highly specialized artifacts per 100 burials, or uses the largest quantity of foreign material." Objections may be briefly stated as follows: The culture may not have been homogeneous; type and quantity of artifacts buried may have varied; important individuals might be accorded greater quantity; foreign materials are not a measure of cultural development; and a given foreign material might be so rare that only a selected few would have it.

A "Tabulation of Artifact Occurrences at Seven Hopewell Sites" is then presented to prove developmental stages. It is difficult to understand how these traits were selected. An analysis of the list reveals so many errors of fact that one would hesitate to draw conclusions from it even if the basic assumptions were accepted. It should be noted that the following traits are not listed for Tremper: cut bear canines, metal-covered buttons, pearls, and effigy animal teeth. A trait listed for Tremper which does not occur is copper crescents. Traits listed for Harness which are absent are copper bracelets and copper crescents. Pearls are not listed for Seip Mound No. 2, and copper crescents are not recorded for Hopewell.

The writers point out that effigy platform pipes at Mound City do not bear out the evidence of development shown by other artifacts and materials, but they apparently attach little weight to that fact. They also state that no cut bear teeth were found at Mound City and Tremper, and that the trait "seems well adapted to be a measure of artistic achievement." "Yet at these two sites otherwise showing

high development, there were no cut bear teeth found." The authors seem undecided about the degree of cultural development at Tremper. They also believe that "cremation can hardly be offered as a reason for the absence of bear teeth at Mound City or at Tremper," yet burned cut bear teeth are present in the collections from both sites.

They give the following time sequence for Ohio Hopewell sites:

Late Hopewell (Hopewell-Turner)
Middle Hopewell (Mound City-Seip)
Early Hopewell (Harness-Tremper)

They think Hopewell and Turner existed throughout the whole time range. They admit that Mound City and Seip were almost on a level with Hopewell and Turner artistically, but place them earlier on the basis of frequency of traits. The authors argue that Seip Mound No. 2 is older than Seip Mound No. 1 because the former has fewer artifacts per 100 burials; also, that Seip Mound No. 2 was built before obsidian or grizzly bear teeth were available. The opposite might be true if the supplies of these substances were exhausted before the mound was started. They claim that the three primary mounds of Seip Mound No. 2 show that Seip Mound No. 1 was begun before Seip Mound No. 2 was completed and that interest shifted to Seip Mound No. 1 so that Seip Mound No. 2. was never completed! They are forced to such an involved theory because they stick to their initial assumptions of artifact frequencies and presence of foreign materials. It is more probable that a people with highly traditional burial customs would complete one mound before starting another at the same location.

They hold that "Harness and Tremper are clearly much the earliest of the group. Many of the high art forms are completely lacking. They had no human effigy forms, only a few animal effigy forms, and they possess many traits now known to appear in Adena, as for example copper crescents and expanded bar gorgets of stone." On the contrary, Tremper had many animal effigy forms, and finely engraved bones with bird designs occurred at Harness. Neither site had copper crescents. Eleven of the thirteen traits absent at Harness are also absent at Mound City, yet Mound City is placed later.

They claim that "all sites begun after the culture complex was well established might be expected to receive all artifact types which the people had ever developed up to that time, even if some of the types were archaic at the time of beginning of the new site." This assumption is not borne out by the evidence.

Assuming each trait "to occur at a certain time interval," a "Chronological Bar Chart" is set up in an attempt to show a time sequence. One has the feeling that this is an extremely subjective procedure. Four of the traits listed as late—metal-covered buttons, pearls, cut bear teeth, and effigy animal teeth—do occur at Tremper. This correction places Tremper as late as Harness. "However, when one considers all the elements of chance involved in the inclusion of an artifact in a site, the variability of human conduct of the occupants of any site, and the imperfection of available information on actual occurrence, the results indicated are all the more surprising." The reviewer concurs, for the conclusions reached are not borne out by the chart. It is important to note that the authors' sequence of sites is the same as would be obtained by arranging the sites according to the total number of traits present at each site.

"Chronology in Adena" may be summed up as follows: Sites with highly developed artifact traits found in Hopewell are late Adena, those without these traits are early Adena. No stratigraphic evidence has yet been recorded.

Adena pottery is discussed by James B. Griffin. He notes that Fayette Thick ware, an early Woodland form in the Northeast, is sometimes associated with Adena-like artifacts. The Adena Plain type is the dominant form in the culture. In Kentucky it is mainly limestone-tempered, in Ohio, largely grit-tempered. Limestone-tempered Copena pottery is similar to Adena pottery, but seems to be later. He suggests that the tetrapodal supports may have originated in the Tchefuncte culture. Adena tablet bird motifs may have developed into Hopewell bird designs on zoned vessels, the bird spreading to the south and zone incising to the north.

Griffin notes that two-thirds of all Ohio Hopewell sherds are cord-marked and one-fifth are plain. Since most Adena sherds are plain, one might hold that a Hopewell site with a high proportion of plain ware was older than one with a majority of cord-marked ware. If this were true, then the sites could be arranged in the following time sequence: Tremper, Mound City, Turner and Seip, Harness, and finally Hopewell. He is quick to point out, however, that this is based on the assumption that plain ware antedates cord-marked ware in the Mississippi Valley, which does not seem to be the case.

Griffin sees some continuity in the pottery of the two cultures, but not in the types which are considered most characteristic of Hopewell. Pottery of both groups is a part of the Woodland ceramic tradition. Little stratigraphic evidence is at hand concerning Adena pottery types, hence the interpretation is not on a firm base. Adena Plain pottery from the Wright Site is closer to Copena pottery than to Ohio Hopewell pottery. Griffin's studies do not support some of the conclusions of Webb and Snow. He finds no evidence of a movement down the Ohio and up the Tennessee; his Hopewell time sequence is different; and he notes that pottery does not furnish evidence of a migration of the Adena peoples from the south.

The study of the physical type of the Adena people, made by Snow, was based on measurements and observations of the remains of 265 individuals. Results are presented in a series of tables and, the more complete skulls are illustrated. The skeletal material was found to represent a homogeneous brachycephalic group of the Centralid type. All available skulls from Ohio Hopewell mounds were also studied. Snow found that 80 per cent of the Hopewell skulls belonged to a dolichocephalic group probably related to the Sylvid type of the Eastern Woodland; that from 10–15 per cent of the Hopewell skulls were similar to the Adena type; and that the remaining 5–10 per cent were undeterminable. The Hopewell long heads were taller and heavier than the Adena broad heads. Occipital deformation occurred in 92 per cent of the Adena skulls and bifrontal in about one-third. Occipital deformation was present in

some of the Hopewell skulls and bifrontal in about 26 per cent. The Copena people were mainly broad-headed, but there was a minor type which was long-headed.

Snow believes that the two types in Hopewell indicate an admixture of Adena and Hopewell peoples. Likewise, that the same deformation types in both groups points to that conclusion. Did the supposed admixture take place before the Hopewell culture had acquired its distinctive characteristics, and, if so, wouldn't a higher degree of admixture be expected? What relation does the broad-headed type in Copena have to the broad-headed type in Hopewell? It has been noted that there are definite Copena traits present in some Ohio Hopewell sites. Isn't it possible that the broad-headed subtype in Hopewell is of non-Adena origin? The fact that the two peoples were of basically different physical types precludes any theory of simple development of one culture from another.

The authors believe the Adena people came from Central America or Mexico; that they brought with them the basic Adena complex of traits; that they mixed with the Hopewell peoples; and that the resultant culture was essentially Adena in origin. Their theory of migration is based on the fact that the people were broad-headed with occipital deformation. The authors' own evidence, as well as ceramic evidence presented by Griffin, seems to show a Northern Woodland origin for the basic Adena complex. They give no evidence to support their statement that Adena man made the first pottery in the area. They say little or nothing about the kind of culture the Hopewell peoples had before they supposedly mixed with the Adena peoples, although they recognize them as old inhabitants whose physical type persisted down into classic Hopewell times. Finally, their interpretation does not consider closely related Hopewellian manifestations in other parts of the Mississippi Valley.

This reviewer recognizes the relationship of Adena to Ohio Hopewell, but does not believe that the authors have presented sufficient evidence to establish the fact that the Hopewell culture grew out of the Adena. The way in which evidence has been presented leaves much to be desired. One is conscious of the repetition of material and of emphatic techniques of presentation—even to the use of bold-face type. There are numerous typographical errors and many of the footnote references are incomplete.

The Adena People is a distinct addition to Ohio Valley prehistory, in so far as it presents a comprehensive and up-to-date analysis of the Adena culture. Snow's study of the physical type presents for the first time a picture of Adena man himself, while Griffin's ceramic study lays the foundation for future research. Webb and his collaborators have advanced several challenging theories; whether or not these theories will stand the test of time remains to be seen.

RICHARD G. MORGAN
Ohio State Museum

The Faulkner Spring Site. MAURICE ROBBINS. (Papers of the Attleboro Museum of Art and History, Number 1, January 1944.)

This is a report by the W. K. Moorehead Chapter of the Massachusetts Archaeological Society on the partial excavation of a site north of Taunton in Bristol County, southeastern Massachusetts. Some twenty-six members of the Chapter participated from time to time in the excavation. The Chapter also had the aid of various specialists, whose assistance is acknowledged in the foreword. As a factual report on the site, it has considerable value, for such detailed objective statements are relatively rare in New England archaeology. This is a contribution to the prehistory of the Northeast of which all concerned can be proud.

The report, written by Maurice Robbins, Chairman of the Chapter, presents the factual data on the site, artifacts, and features and makes a modest attempt to orient the site in the general northeastern culture picture. The major sections of the report are on the general region; the history of the project; the surface features; methods and records; general features of the site; description of the artifacts; and conclusions. Of particular interest was the recognition of a rectangular and a circular habitation floor with associated post-mold patterns. A table gives the length, diameter, and shape of the fifty-three post molds on the site. Some forty-five pits and sixty-four hearths were uncovered in the 1036 square yards excavated. The pits were distinguished from the hearths by their depths of over twenty centimeters, although there was little difference in the contents of the two classes of features. At least one of the functions of the pits was to receive the contents of hearths. There are figures illustrating representative pits and hearths and tables presenting their measurements and the features associated with them. Artifacts were obtained from sixteen pits and four hearths.

There were three areas of large round pebbles of various materials, under one of which there were small black deposits with indications of calcined bone. Neither the stones nor the soil around them showed any evidence of fire. In passing, it might be mentioned that the shallow stone layers were not in effigy shapes. Careful excavation techniques resulted in the recovery of six large stone anvils with an association of rubbing stones, polishing stones, hammerstones, and many thousand flakes of quartz, felsite, and shale.

Description, illustrations, tables, and classifications present the details regarding the various artifacts, which include a predominance of narrow stemmed and side-notched projectile points; other artifacts are scrapers, an ulu, drills, adzes, gouges, pestles, hammers, grinders, mortars and pestles, choppers, a plummet, a bipennate bannerstone, and other miscellaneous objects. Only three fragments of pottery were found. The animal bones were plentiful but badly decayed and the fragments were small. A modern fauna is indicated. There was no evidence of agriculture, although carbonized organic material included acorns, hickory nuts, water lily seeds, fragments of alder, chokeberry, and white pine branches. The absence of evidence of corn may be correlated with the absence of the chipped hoe, which is fairly common on later (?) sites in this area.

Robbins concludes that the site is the product of a people with a hunting-fishing-food gathering economy. The absence of pottery and steatite and the presence of many fire-cracked stones suggest that food boiling was done in wood or bark containers. Many of the implements were

manufactured in the village area, and the shelters were small and relatively impermanent. There is no knowledge of the burial practices or of the typical type.

On the basis of a brief trait table, it is apparent that the Faulkner Spring site has more resemblance to eastern Laurentian sites than to sites of the Orient focus of the Coastal aspect. There is a brief comparative section in which Robbins classifies the Faulkner Spring projectiles according to the system used by Dr. Cross in Volume 1 of *The Archaeology of New Jersey*. He presents evidence on the grouping of the types of projectile points and their association with pottery which suggests that there is more cultural (and chronological?) variation in New Jersey sites than Dr. Cross recognized. A curious fact is the absence of cultural comparisons with sites on Martha's Vineyard or other eastern New England complexes.

The report would have benefited by editorial supervision which would have provided a table of contents with a listing of plates, figures, plans, and tables. Captions are missing from plates and some figures.

It is to be hoped that there will be future reports from the various chapters of the Massachusetts Society for, as is stated in the preface, "it is possible for non-professional archaeologists, without special training or financial assistance, to carry out a major excavation." Other northeastern archaeological societies along with that in Massachusetts are providing extremely valuable data for the unravelling of the cultural history of the area. These reports are evidence of the interest of the society members in cultural reconstruction and determination of interrelationships, which are among the primary aims of the professional. In that endeavor there is no need for a classification of archaeologists save as to the manner in which they obtain their livelihood.

JAMES B. GRIFFIN
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Catawba Pottery-Making, with Notes on Pamunkey Pottery-Making, Cherokee Pottery-Making, and Coiling. VLADMIR J. FEWKES. (Proceedings of the American Philosophical Society Vol. 88, Number 2, pp. 69–124, 33 text figures, map. Philadelphia, 1944).

As a contribution to the study of Southeastern native ceramics, this paper by the late Dr. Fewkes is most valuable. It is based upon field work which began in 1929 and continued until August, 1941, about four months before his death.

The major portion of the paper deals with objective observations on the methods and techniques employed by contemporary Catawba potters using native processes. Twelve of the most competent potters furnished the data which Fewkes has presented with excellent organization. The processes described begin with the acquisition and treatment of raw materials and continue through all the details of manufacture, including post-firing treatment. The portions dealing with building technique, surface finish, and firing are especially noteworthy.

In describing the building techniques of Catawba potters, Fewkes offers the term "segmental building" as a substitute for the general term "coiling." The justification for this is more apparent after a reading of the addendum on coiling in which the author examines critically the looseness with which the term "coiling" has been employed in the literature on pottery. Since Catawba potters do not do spiral coiling but, rather, use separate fillets, either as prepared rings or bonded as the construction proceeds, the author sought for a suitable term to distinguish between the spiral and annular variants. Whether or not the pottery technologists accept Fewke's term, he has amply demonstrated the need for a more precise terminology and his terms and classification of construction techniques are simple and clear. This reviewer is particularly happy to understand finally the difference between what Southwestern archaeologists call Anasazi "coiling" and Hohokam "paddle and anvil" methods.

In the discussion of surface finish, there is an interesting observation regarding the effect of pebble polishing on the surface of pottery. Fewkes noted the increased textural density which simulates a slip. This phenomenon is frequently observable on prehistoric Southeastern pottery on which true slips were seldom applied.

Commenting on the mottled color of Catawba pottery, the author states that it is considered an esthetic asset. Thus, he found it difficult to describe the color, not only of the ware as a whole, but of individual vessels. Southeastern archaeologists are familiar with the difficulties of describing the color of pottery types in the area because of the great variation present. If the aborigines deliberately sought to enhance their pottery through color variation, as do the Catawbas, perhaps we need not worry too much about the technicalities of color description.

The notes on Cherokee pottery-making are most significant with respect to the rapidity and ease with which the Cherokee were acculturated by Catawba potters in recent times. To one who has wrestled with the problems of "presumed" Cherokee pottery, this is thought-provoking. Furthermore, it raises the question as to the origins of Catawba pottery-making itself. There seems to be a discrepancy in the interpretations of Fewkes, Coe, and Griffin as to what constitutes ancient eastern Siouan pottery. Fewkes states on page 71 that the pottery found on sites in North Carolina, investigated under the direction of Joffre Coe, included "mottled polished ware which in construction, surfacing and firing closely resembles the modern Catawba product. Coe, on the other hand, has stated at various times that the typical eastern Siouan pottery has surfaces roughened by a cord-wrapped or check-carved paddle. Griffin, in a recent article,[1] discussed pottery of the Carolina Sioux and indicates that most of it was roughened in various ways but that some was smoothed. Griffin's conclusion is that the pottery of the Siouan tribes belongs among the Woodland wares.

Is one to assume that the ancient Catawba ware was an exception among eastern Siouan archaeological materials? Fewkes' statement would seem to indicate that such is the case. Or is it possible that recent Catawba pottery represents an acquired tradition? Since its present character

[1] AMERICAN ANTIQUITY, Vol. X, No. 4 (April 1945), pp. 321–330.

corresponds more closely to Mississippi than to Woodland wares, one questions the advisability of accepting Fewke's assumption that the methods observed for the contemporary Catawba can be considered as a "case of cultural continuity which demonstrates the role of a direct traditional survival." It seems very likely that long before Palmer's visit in 1884 the Catawba had borrowed methods and techniques of pottery-making that were widely distributed in the Southeast. To attempt an historical reconstruction on the basis of either Fewke's or Palmer's data is risky unless acculturation is recognized as the powerful factor it undeniably was in Southeastern prehistory.

This criticism of a minor part of Fewke's paper detracts nothing from the value of the paper as a whole. The factual data presented and the contributions to objective methodology represent a work of scientific merit which will be recognized by all workers in the field of archaeology.

MADELINE KNEBERG
University of Tennessee

The Iroquois: A Study in Cultural Evolution. FRANK G. SPECK. (Cranbrook Institute of Science, Bulletin No. 23, 94 pages, illustrations, Bloomfield Hills, Michigan, 1945, $1.00.)

The Celestial Bear Comes Down to Earth: The Bear Sacrifice Ceremony of the Munsee-Mahican in Canada as Related by Nekatcit. FRANK G. SPECK in collaboration with JESSIE MOSES. (Reading Public Museum and Art Gallery, Scientific Publications, Number 7, 91 pages, 4 plates, 9 figs., Reading, Pennsylvania, 1945.)

From the prolific and learned pen of Dr. Speck we have two more excellent monographs. His colleagues and friends will never cease to be amazed at the abundance of material this energetic scholar is able to bring forth.

The Iroquois is a handbook, profusely illustrated with photographs from the extensive Cranbrook collections and written primarily for the intelligent layman. Professional workers in the field of Northeastern cultural history will, however, find it extremely useful, as many facts not hitherto brought together and many problems not before summarized are herein presented. In the "Introduction" it is pointed out that "the old, sombre picture of the Iroquois as ruthless barbarians is beginning to fade" and that a more accurate picture of the entire panorama of Iroquois culture, including their important roles as agriculturists and religious ritualists, is now being presented. The section entitled "The Iroquois Linguistic Family and Populations" contains an invaluable summary of the areas inhabited by various Iroquois groups and their numbers when first discovered and at the present time. The list of the reservations occupied at the present time by the original five tribes of the Confederacy and the population at each reservation, by tribes, should prove invaluable to the field worker, as this information has never before been brought together in an easily accessible form.

The most important phases of Iroquois life are skilfully summarized in the remaining sections. In "Social and Civil Aspects of Iroquois Culture," the political organization of the Five Nations is discussed, and the clan and phratry subdivisions of the northern and southern Iroquois tribes are tabulated. The social organization; the relationship of the confederated tribes, one to another; and the symbolism of the league as a magnified Long House are described. The Iroquois contributions to agriculture and their uses of elm bark and other natural products are set forth in "Economic and Ecologic Aspects of Iroquois Culture." The next two sections, "Iroquois Arts and Crafts" and "Iroquois Decorative Designs and Symbolism," deal with the more artistic products of the material culture and the decoration applied to such things. The reviewer agrees heartily with the author that Iroquois crafts, from a comparative standpoint, do not represent as low a plane of workmanship as several other writers seem to believe. Throughout Dr. Speck's authoritative discussion of the transitions in Iroquois arts, crafts, and designs, he points out many problems that still await the research of the ethnographer. The final chapter, "Ceremonial Properties of the Iroquois," contains brief descriptions of all the physical paraphernalia of their religion—masks, musical instruments, wampum, etc.—together with short sketches of their medicine societies and modern form of worship, as revealed by Handsome Lake, the prophet. This monograph should serve well as an informative account for the layman, as an aid to the professional, and perhaps as a textbook for the student.

In *The Celestial Bear Comes Down to Earth*, Dr. Speck has produced a remarkable document. By great good fortune, he met at Six Nations Reserve, Nekatcit, the only individual who remembered the Bear sacrifice ceremony of the Canadian Delaware. The information acquired from this person, combined with Speck's own vast knowledge of the Delaware and with considerable skillful detective work, has produced a monograph which will be a standard reference for all those interested in the religious aspects of the Eastern Woodland Indians' culture. The text was obtained from Nekatcit just in time, for the great informant died shortly thereafter. Aside from the Nekatcit text and discussion of the ceremony, the book contains a wealth of other information—for instance, the account of the romantic Montour family, the ethnic composition of the Canadian band of Delaware, and a discussion of their dialect. This book and the previous studies of Delaware Big House ceremonies in Canada and Oklahoma, by Speck and Harrington, now comprise a body of material from which significant conclusions may be drawn regarding the religion of the Delaware group of tribes before their dispersal. Quite incidentally, this work also demonstrates that the ethnologist must frequently be as much a first-class detective in reconstructing the recent past in the eastern area as is the archaeologist in reconstructing somewhat more remote periods.

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Pumice Beds at Summer Lake, Oregon. IRA S. ALLISON. (Bulletin of the Geological Society of America, Volume 56, pp. 789–808, 1945.)

Summer Lake is a shallow alkaline body of water in a

closed basin in semiarid south-central Oregon. Distinct abandoned shore lines indicate deep water in the basin contemporaneous with ice sheets in western Canada and large glaciers in the western mountains. The modern lake stands normally about 4145 feet above the sea, but the highest recorded modern level is 4178 feet (p. 791). The highest abandoned shore line is about 355 feet above the present lake, or at 4500 feet. This old shore is attributed to the Bonneville Pluvial, the correlative of the Tahoe-Iowan Glacial, the first stage of the Wisconsin glaciation. It is perhaps some 65,000 years old. Another well-developed shore line stands about 215 feet above the modern lake, i.e., at an altitude of 4360 feet. This may be of Provo age and correspond to the Tioga-Mankato Glacial, the last large glacial culmination in western North America. The Provo shore is probably about 23,000 years old. A third sandy beach, 45 feet above the modern level (12 feet above the highest modern stand), or at 4190 feet, may be a few thousand years old (p. 794).

In bluffs on the Ana River, just north of Summer Lake, there are layers of pumice from eruptions of Mt. Mazama, which led to the formation of Crater Lake sixty-six miles to the west, and from a later eruption of Newberry Crater, fifty-five miles to the north-northwest (pp. 789, 795, 800). The pumice layers occur below and between beds of clay, silt, and sand and were consequently deposited in water. The Mt. Mazama pumice, found at an elevation of about 4225 feet, could have been laid down while the water level stood at any altitude between 4245 and 4230 feet, or 100 to 85 feet above the modern lake (p. 800). Since the last pluvial lake culminated probably some 23,000 years ago and had diminished to the size of the modern lake some 10,000 years ago, the shore could have fluctuated between the levels of 4245 and 4230 feet from before to considerably after 14,000 years ago (p. 801). Other estimates date the Mt. Mazama pumice at about 13,500 and 10,000 years (pp. 802, 803). This pumice probably is over 10,000 years old.

This new date for the Mt. Mazama pumice is archaeologically significant, for the pumice overlies one site at Wikiup, sixty miles north of Crater Lake,[1] and three sites in caves near Paisley, east of the south end of Summer Lake,[2] all described by L. S. Cressman. These sites may thus be more than 10,000 years old, which is in good agreement with the presence of bones of extinct horses and camels in one of the cave sites. The purity of the pumice layers in the caves and the presence of heavy minerals among the lighter glass speaks against secondary eolian origin, but is in accord with Allison's suggestion that the pumice fell on the slope above the caves, perhaps on a snow crust, tumbled and rolled down, and was blown only from the cave mouths into the caves, forming the pumice beds shortly after the eruptions (p. 805).

If charring of fiber sandals in the Fort Rock Cave, thirty-five miles north-northwest of Summer Lake, occurred because the overlying Newberry pumice was hot when it fell, man lived in this cave a couple of thousand years after the Mt. Mazama eruptions, or perhaps as long as 10,000 years ago, since the Newberry pumice occurs near the top of the profile on the Ana River.

Allison's study is of further archaeological import in that it gives the first exact relationship between the lake levels during the Provo (last) Pluvial and the somewhat moist age a few thousand years ago. As stated, these levels stood 215 and 45 feet respectively, above modern Summer Lake.

The shore lines and pumice layers on Summer Lake present an exceptional combination of important data, which have been expertly presented and applied by Allison.[3]

ERNST ANTEVS
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Arte Precolombino de Mexico y de la America Central. SALVADOR TOSCANO. (Instituto de Investigaciones Estéticas. xviii+557 pp. Illustrated. Mexico, 1944.)

Salvador Toscano, who has already made a name for himself in the field of Colonial art in Guatemala and his native Mexico, appears here with his first book. At the outset it may be said that it is a great book, not only in size —it weighs 6½ pounds—but in the richness of its material, its method of presentation, and in the superior quality and wide scope of its illustrations.

In a preliminary note, the author states that he personally visited the various archaeological zones, some of them repeatedly, during the years 1937–1942. These sites, however, were restricted to the Mexican and Maya areas, notwithstanding the more extensive territory suggested by the title; with the exception of one stone figure from Costa Rica, no other Central American culture is illustrated.

The author has divided his material into nine parts, the first of which deals with "La Estética Indigena." In this chapter, he reviews the essence of the archaeological styles, proffering the statements of a long list of authorities, from Winckelmann and Lessing to Venturi and Lunatcharsky. This discussion is followed by an explanation of the "terrible" and the "sublime" in the "archaic" arts (i.e., pre-Columbian), and then, later, in another sub-chapter, he contributes his own interpretation of the beauty apparent in the great cultures.

In the second chapter, the reader is given a glimpse of the historical setting, grouped under the headings Maya, Mexican High-Plateau, Atlantic Littoral, Zapotec and Mixtec, Tarascan and Colima. The remaining chapters in the book are devoted to the arts themselves—architecture, sculpture, painting, pottery, mosaics, feather-work, and jewelry. About 320 illustrations (according to the reviewer's cursory count, for there is no list of illustrations, nor are the pictures numbered) assist the reader in visualizing the fine and applied arts of Mexican and Maya areas.

The descriptions are naturally often sober, but when it comes to appreciation Toscano discloses his own individual

[1] AMERICAN ANTIQUITY, Vol. 3, 1937, pp. 53–67.

[2] University of Oregon, Studies in Anthropology No. 3, 1940; Carnegie Institution of Washington, Publication 538, 1942, pp. 21, 25, 93; *Scientific Monthly*, January, 1946).

[3] Ed. Note—After this review was completed, Dr. Allison's paper entitled "Pluvial Lakes and Pumice" appeared in *The Scientific Monthly*, Vol. LXII, No. 1, as part of the symposium on Early Man in Oregon.

enthusiasm, which comes through in well-written sentences, inspired and vigorous. With this capacity, it is all the more regrettable that he did not work the illustrations into the text, figure by figure. The text reveals long hours of meditation on the complex esthetic problems involved, as well as thorough familiarity with the basic works on pre-Columbian archaeology. It is also good to see in the bibliography the wide field and many nations from which Toscano selected his titles (in contrast to some of the authors quoted—especially the French—whose ignorance of English affects the up-to-dateness of their writings).

To return to a more detailed discussion of the work, in "La Muerte de los Estilos," the last section of the first chapter, Toscano goes into an esthetic evaluation so controversial that it can scarcely be touched upon within the limitations of a book review. A remark, however, should be made on his statement that elements of dissolution exist in the Baroque. Just seven pages earlier he speaks with great reverence of Riegl; it is doubtful if the great Austrian art historian, clarifier of the just values of Baroque, would agree with him. Turning from the Christian Baroque, Toscano makes the statement that Palenque "represents the purest epoch of moderate Baroque," while Kabá, Uxmal, and Chichén are declared to be "refined ultra-Baroque." One would expect the descriptive term "Baroque," whether applied to Christian or pre-Columbian architecture, to imply, among other characteristics, a flowing richness of continuous all-over decoration and much vertical as well as horizontal movement. Where are these "ultra-Baroque" features to be found in such edifices as the Temple of the Three Lintels in Old Chichén, El Castillo, or the Temple of the Warriors?

Toscano is searching sincerely for the beauty in pre-Columbian art and trying to communicate it to others. As a son of Mexico, representative of a modern culture in the Western Hemisphere which is now coming into its full power, he could have afforded to adopt a more independent attitude in his approach than he does. Instead, while siding against the school of "romantic" Indianists of the last century, who held that the pre-Columbian civilization was cut off in its flower, he takes up with another nineteenth-century school, just as dated, declaring these early cultures "fragile" (a quote from Hegel) and "pulverized" (a quote from Gobineau), without will or determination to live. This manner of thinking, which established an exaggerated admiration for the classic world and the Greco-Roman ideal of beauty, and from which the appreciation of pre-Columbian art still suffers, came no nearer a true evaluation of the native cultures than did the "romanticismo indianista" which he deplores. While no one would claim that the pre-Columbian cultures were at their zenith at the time of the Conquest, is it not probable that the Indians would have gone on creating still more great art if they had not been subjugated by the white man? How much artistic individuality they retained for a long time after Spanish domination is evidenced by their metal-work, weaving, and pottery in the Transition Period. Furthermore, that they still possess vitality and inventiveness in the arts, after more than four hundred years of subordination, cannot be doubted by anyone who has seen their markets in the Guatemalan highlands or the Peruvian and Bolivian Andes.

From numerous references, it would seem that Toscano is a disciple of Spenglerism. He closes his first chapter with a quotation from Spengler, and again, in the chapter on architecture, we read in another quotation that Tenochtitlan compared (in its "brutalized forms of life," p. 76) with Bagdad and Babylon of the ancient world and with London and Paris of today. The cities of the Near East have at least something in common with this ancient Mexican metropolis in that they were religious centers where priesthood's power was paramount. But to link Tenochtitlan with London and Paris shows only how forced is the inclusion of Spengler at this point. Tenochtitlan and other marvels of pre-Columbian civilization could better be evaluated with quotations from other sources, if quotations were at all necessary. It weakens the author's whole effort to rely so heavily on European philosophers who never occupied themselves to any degree with the cultures of the New World. Hegel, also much quoted, for whom "America was nothing but the echo of the Old World," is a case in point. One wonders whether an approach weighted down with such progressively decaying German philosophy can be the right one for a just evaluation of the pre-Columbian civilization which developed in isolation and which in many ways cannot be measured with the instruments that Europe has contrived.

The sifting and selecting of material for this survey presented a difficult task, and most of its problems were successfully solved. However, some omissions were noted. In the first place, under sculpture, no illustration is offered of any Maya Maize-god, a type with universal value and appeal. Also, the most beautiful and perfect example of this type—in the British Museum—is here still referred to as "la muchacha cantando," in spite of the fact that it has been known as the Maize-god ever since the Catalogue of the Maudslay Collection was issued in 1923. Then, no example of atlatls with their intricate wood-carving is included, although four wooden drums are illustrated. Neither is there any mention of the once colorful and technically amazing textiles. It is true that no representative example survives, but in a book on the arts of these peoples some mention of that craft, as recorded on stelae, in wall-painting and pottery, would not have been out of place. Another important and especially individual artistic manifestation of the pre-Columbian cultures, their jade carving, appears somewhat neglected. Only seven illustrations show jade objects, and only two of these are Maya. Although Toscano points out the reference to this material in the Tribute Roll, he speaks of the origin of this stone as an archaeological problem. It was a problem at one time, but its autochthony in our own Rocky Mountains was definitely established some years ago, its presence in Guatemala is proven, and, when geological investigations on a larger scale are possible in Mexico, the results may be expected to be similar there. Boulders of high quality jade weighing as much as 3300 pounds have been discovered near Lander, Wyoming, where for more than a decade some of the population have made a living from the sale of carved jade objects. The Natural History Museum of Chicago also has a

piece, weighing more than a ton, from this same region. In 1937, a block of jade, approaching two hundred pounds in weight, was excavated by the Carnegie Institution at Kaminaljuyú and now is in the Archaeological Museum of Guatemala City. Finally, in the chapter on pottery, the absence of measurements—noticeable throughout the book—makes itself especially felt, since to the nonexpert the visualization of plastic effects is very difficult without an idea of the size.

Some minor inaccuracies were observed in the captions: the Copan vase (p. 414) should be listed as in the Museum of Natural History, New York; the Maya figurine (p. 417) is in the Fogg Art Museum, Cambridge; the gold pendant (p. 531) was in the possession of the Museum of the American Indian, New York.

However, the real quality of the book, in text and pictures, far outbalances its shortcomings. Special appreciation is due Toscano for the great number of photographs he made himself, doubtless at considerable effort and cost. That of the Maya sacerdote (p. 389) is a prize piece, showing the author's innate sensitivity toward the subject and his command of the apparatus. One of the greatest values of the book is perhaps the extended treatment given painting; all the extant murals are presented, and also the known codices are discussed at considerable length, from their earliest history to their value as contributions to our present knowledge. Another distinctive feature is the author's new and excellent idea in the sub-chapter on columns, which is well presented and illustrated.

Praiseworthy indeed is the enterprise of the Instituto de Investigaciones Estéticas in bringing out such an important book in wartime, on fine paper, in most readable type, and with such good quality of illustrations. From the prologue by Professor Manuel Toussaint, distinguished doyen of art history in Mexico, we learn with pleasure that Toscano's volume is the first in a planned trilogy and will be followed by one on Colonial art, by Toussaint himself, and by another on modern Mexican art, by Justino Fernández. Not only Toscano but Professor Toussaint and all others who collaborated on this notable achievement are to be congratulated.

Finally, a few words should be added about the book jacket—lately become the conveyor of worthwhile information and opinion not to be found in the book itself. In this case, Dr. Alfonso Caso, the universally esteemed chief of Mexican archaeology, gives the work a recommendation of over six hundred words, identical with his book review published in the Institute's *Annales*. In a straightforward expression of appreciation, he acknowledges the scholarship in the United States and the pioneer work it has done also in appraising the artistic side of Mexican cultures. His attitude is especially heartening to this reviewer, who has just returned from an extended survey in South America, where he was sometimes struck by the lack of comparative material and synthesized knowledge and by a distrust of the United States, still the "octopus of the North," in certain circles. The appearance now of Toscano's book, with its Spanish text and rich illustrations, seems to be especially timely. It should serve to break down the barriers of local circles and to disseminate an awareness of the vastness of the pre-Columbian field and a knowledge of the comparative values of its cultures. For our part, we in this country must persevere in our co-operation, regardless of the fact that budgets are being slashed right and left, to prove that our interest is sincere and that our good will is not seasonal.

PÁL KELEMEN
Norfolk, Connecticut

The Indian Background of Colonial Yucatan. RALPH L. ROYS. Carnegie Institution, Publication 548. 244 pp. 6 maps, 23 figs. Washington, 1943. Paper, $1.75, cloth, $2.75.

Mr. Roys, one of the leading Maya linguists and archaeologists, presents a new book on a very interesting subject. It is carefully written and contains an uncommon amount of information compiled from different sources. The wealth of material on which he has drawn can be appreciated by the list of references. His language is clear, concise, and readable. This is not a dry, scientific book for the scholar, but one which will appeal to the general reader, who seeks instruction in a brief and condensed form. The author's main object is to present the reaction of the Maya people against the influence of Spanish rule by the friars and the military government in the Colonial period.

The chieftains having been provided with the *Ordenanzas* of Tomás López in 1552, later improved by Don Diego García de Palacios, in 1575, were left in power under the vigilance of both the friars and the military. They accomplished what General MacArthur is trying to accomplish with the Japanese people. Men always act in a similar manner under the same circumstances.

Roys draws from Lundell in the first chapter, "The Country," and describes the Peninsula of Yucatán with the aid of an excellent map, drawing on the work of Shattuck (1933).

Other chapters are: 2, First Impressions; 3, Towns and Buildings; 4, Physical Appearance and Costume; 5, Manners and Customs; 6, Social Organization and Land Tenure; 7, Agriculture and Food; 8, Industry and Commerce; 9, Political Institutions; 10, Warfare; 11, Religion; 12, Science and Learning; 13, Neighbors of the Yucatecan Maya: Tabasco, Zoque, Chol, and others; Hibueras-Honduras (with maps); 14, Bibliographical Sketch; 15, Preservation of Indian Nobility (with an Indian map of Yucatan showing the Xiu area); 16, The Cacique System (the leading and original parts of Roys' book); 17, Functions and Powers of the Cacique; 18, compensation of the Cacique; 19, Privileges of Caciques; 20, Succession of Caciques; 21, Women Caciques; 22, Indian Nobles and the Native Militia of Yucatán; 23, The Governor Compared with the pre-Spanish *Batab;* 24, Conclusions.

The author concludes that the Xiu *Probanzas* are the only source of information concerning the history of the native aristocracy. The late Mr. T. A. Willard secured in Yucatan some valuable papers of the Cocom family as noblemen, setting forth the names of all members of the family who ruled in Mayapan, together with the length of each one's tenure of office. Part of this information he published in *The Bearded Conqueror*, 1941, Chapter 13.

The book closes with photographs of archaeological

monuments, modern houses, and people of Yucatan. There is also a glossary of Maya and Spanish terms used, a list of references, and an index.

In the Appendix, entitled "The Land Treaty of Mani," are included the Mani Document, the Agreement between Ticul and Ppuztunich, and the Calotmul Document, accompanied by maps of the province of Mani taken from the Xiu Chronicles and the Cronica de Mani. These have been carefully translated from the Maya by the author, in which work he is a specialist. He has done his best as a scholar who learned the Maya language from books, but the knowledge of the language gained in this way is different from that gained by learning from childhood the spoken language used by Indian nurses in the land of the people. Maya is different from any other language, either spoken or written, in that there is a fundamental meaning to every word, but scholars allow themselves many liberties concerning Maya words—"learned trifling," Brinton has termed it—like *Calakmul, Uaxactun, tun, hotun, lahun, tun, baktun, pictun, Tzolkin,* and so forth.

At the first meeting of all the Maya caciques in 1557, called to decide the boundaries of Mani, Don Juan Cocom (Nachi Cocom), the Governor of Sotuta, was present; he did not appear at the second meeting. Word was sent that he had died of disease—*cimil u cah chápahan,* in the Maya text.

France V. Schols and Eleanor B. Adams have published a very interesting book, *Don Diego Quijada, Alcalde Mayor de Yucatán, 1561–1565.* In this book we have positive statements about the *auto-de-fé* performed at Mani in 1562 by Friar Diego de Landa—an act which has brought a curse on his memory by archaeologists because it destroyed all the evidence of pre-Spanish Maya books and idols and eradicated every vestige of calendar, incantations, rites, and ceremonies formerly contained in the books.

We must consider that Nachi Cocom, the Maya chieftain baptized under the name of Don Juan Cocom, became very ill in 1552 and decided to make sacrifices to the Maya gods to relieve him of his sufferings. Information regarding the sacrifice is contained in Schols and Adams, Volume 1, Chapter 12, where, on page 80, information given by the witness Melchor Canché is cited. Before the apostolic notary, Juan de Villagómez, Canché deposed, on the nineteenth of August, 1562, that on going to church at midnight, about five years ago, more or less, to pray matins with the other school boys of said town of Sotuta, he saw how the caciques and the most prominent citizens of the town were performing a sacrifice of two young girls; that the girls, whose hearts had been opened previously, were killed and tied to two wooden crosses that were made for the purpose; that the persons who tied both girls to the crosses were Lorenzo Cocom and Juan Cimé, and heard them say, "here you see the figure of Jesus Christ"; that Don Juan Cocom (Nachi Cocom), the Governor of Sotuta was present, as were a number of other prominent persons whom he enumerates; that after this sacrifice, about five months later, he saw how they sacrificed again, two boys, and after the boys were killed how they offered their hearts to the idols; this also was done by Lorenzo Cocom and Juan Cimé; that after this sacrifice they forgot to continue said sacrifices; that the witness did not see another until a year ago (1561), when the hurricane blew, when another sacrifice was performed by the same people in the house of Lorenzo Cocom; Don Juan Cocom had died; that he saw how they sacrificed two other young girls which were placed, tied up, on two crosses; that the said Lorenzo Cocom and Juan Cimé did the tying, saying that these girls were the daughters of their god; that in saying these words they brought them down from the crosses, opened their hearts (alive) and offered them to the idols; that said Lorenzo Cocom was a Maya priest *ah-kin;* and that the bodies were thrown into the same *cenote* or well as the previous sacrifice. . . . Antonio Pech (page 78) made a deposition similar to that of Melchor Canche, adding that they, the *ah-kins* said, "Let these girls die fixed to the cross as Jesus Christ died, who they say was Our Lord, but we do not know he was."

From these statements we can conclude that, after the death of Nachi Cocom (Don Juan Cocom), there was no real need to pray to the idols for his health. The murders of boys and girls stopped for several years until the hurricane blew, when another religious spell induced Don Lorenzo to continue, this time for his own good and to promote his personal prestige as *ah-kin* and governor of Sotuta.

We can now imagine how indignant Friar Landa was. His religious zeal mounted. He assumed the powers of Inquisitor, put people in jail and tortured many with the aid of Governor Quijada—Lorenzo Cocom committed suicide—destroyed their idols and books, and endangered the lives and properties of the Spanish *encomenderos* and residents by provoking a general Indian rebellion which they could not possibly control. So they complained to Spain. Landa was indicted, as was also *residenciado* Governor Quijada.

Landa's judges acquitted him. To exonerate himself he offered numerous depositions, including those mentioned, and wrote in 1566 his famous *Relación de las Cosas de Yucatán,* the old authority of all Maya archaeologists which is used to advantage by Roys in describing the ritual ceremonies, customs, calendar, and so forth. This information shows that the ugly, brutal, and bloody rites described were introduced into Yucatan by the Nahua or Mexican captain and High Priest, Kukulcan (Quetzalcohuatl), as sacrifices offered to the Sun God Xiutecutli, and were put in force in Chichen Itza. Evidence for this is seen in the *tzompantli,* still in existence in these ruins. The old Maya Indians did not have them before.

Fray Bartolomé de las Casas had aroused indignation all over America over the way in which the conquerors had been abusing the Indians. Landa was acquitted and the King of Spain, Phillip II, had him appointed Bishop and sent him back of Yucatan to put things in order, probably thinking that he had put the "right man in the right place."

JUAN MARTÍNEZ HERNÁNDEZ
Merida, Yucatán

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NOTES AND NEWS

MEETINGS APPROVED

The Executive Committee has unanimously approved a special meeting of the Society for American Archaeology in conjunction with the annual meeting of the American Anthropological Association to be held in Chicago during Christmas week, 1946.

The Council has approved Ann Arbor, Michigan, as the place of the 1947 annual meeting of the Society for American Archaeology subject to the selection of a suitable date in May. This will be a joint meeting with the Central States Branch of the American Anthropological Association.

ERRATUM

Through a regrettable error on page 229 of the April number (Vol. XI, No. 3), it appears that the author has no liking for the detail involved in preparing his report. The first sentence of the paragraph beginning at the bottom of the left-hand column and continuing to the top of the right-hand column appeared as follows:

"The method of presentation employed in this report leaves much to be despised in the way of detail."

As it appeared in the manuscript, this sentence read:

"The method of presentation employed in this report leaves much to be desired in the way of detail."

Sincere apologies to the author for this error which crept in after he had read and approved proof.

ARCTIC AREA

Archaeological field work in 1945 was limited to Helge Larsen's excavations in the Aleutian Islands, which were carried on with the cooperation and assistance of Naval personnel from August 3 to September 16. The first site worked was a large midden (Site D) on the west side of Amaknak Island (Dutch Harbor) which had been bisected by a road and partly leveled by construction work. This is the same site from which George Quimby described two periods of art—the older closely resembling Dorset and the later Punuk-like—on the basis of material excavated by Lt. Comdr. A. R. Cahn, U.S.N.R. (AMERICAN ANTIQUITY, vol. XI, No. 2, pp. 76–79). Larsen's excavations were confined to the 12 to 14 feet of undisturbed material, and yielded 7 skeletons and a large collection of artifacts. Two of the skulls had labrets in place and another skeleton was partly covered with red ocher.

One objective of the excavation had been to check on a supposed connection between prehistoric Aleut culture and the old Ipiutak culture of the Arctic Coast. Culture elements proving such a connection are reported to have been found, especially in the lower layers of the midden.

Limited excavations were also made in a large midden at Eider Point, on the west shore of Unalaska Bay, and at nearby Nateekin Bay, where several house pits, apparently very old, but containing only a very thin culture layer, were found on the top of a hill.

A total of 2481 specimens were found at the Amaknak and Eider Point sites. The results of Larsen's excavations support previous indications that the Aleutians were densely populated in pre-Russian times and that the early culture was more Eskimo like than the later. As reported by Hrdlička, implements of chipped stone were found to be more numerous in the lower levels, rubbed slate in the upper. There is also confirmation of Quimby's statement that decorated objects occur more frequently toward the bottom of the midden.

Soon after returning to the American Museum, Mr. Larsen left for Denmark where he resumes his work at the National Museum in Copenhagen.

Dr. Erik Holtved of the University of Copenhagen has returned for another year's work in the Thule district of Northwest Greenland. His two-volume report, "Archaeological Investigations in the Thule District," which appeared in 1944, as vol. 141, Nos. 1 and 2 of the *Meddelelser om Grønland*, describes a succession of culture stages and definitely establishes the Dorset as the underlying culture.

L. J. Giddings, Jr., will spend the summer collecting tree-ring material in the Arctic for the University of Alaska. After descending the Mackenzie River by boat, he will work along the Arctic coast to the west, returning to Fairbanks in time for the opening of the University in the Fall.

Dr. Douglas Leechman of the National Museum of Canada left in May for a season of archeological and ethnological work on the upper Yukon and Porcupine Rivers.

Margaret Lantis plans to return to Alaska this summer to obtain additional ethnological data on the Nunivak Island Eskimo. Dr. Lantis has received a grant from the Arctic Institute of North America to enable her to complete her Nunivak study, the first volume of which appeared in March 1946 ("The Social Culture of the Nunivak Eskimo," Trans. Amer. Phil. Soc., vol. 35, Pt. 3).

NORTHEASTERN AREA

The Massachusetts Archaeological Society reports a series of digs under the auspices of several chapters. As we go to press the most promising seems to be that conducted by the Warren King Moorehead Chapter, in the township of Bridgewater. The site is so situated that it is conceivable that it was the village visited by Stephen Hopkins and Edward Winslow on their journey from the settlement at Plymouth to Massasoit's headquarters at Pokanoket. To date there is not sufficient evidence from the dig to warrant any further statements about the site.

A report on excavations by the same chapter at the Ford site, in Norton, where gun flints, musket balls, and banner stones were uncovered, will appear in the October number of AMERICAN ANTIQUITY.

Plans for summer work on the part of the Robert S. Peabody Foundation for Archaeology look toward the following out of leads already uncovered. Mr. Frederick Johnson is watching carefully the excavation for the foundation of the new John Hancock Life Insurance building in Boston, but 800 feet from the site of the Boylston Street Fishweir. Excavations here promise to uncover the same sediments, and, possibly, even sections of the fishweir itself. Plans have been made to continue investigations of problems stated in some detail in the report published in 1942. It is proposed to pursue additional analyses of the wood recovered, and, if possible, add certain bacteriological investigations. A geological study of the site and the surrounding region is projected and arrangements have been made for the study of foraminifera, diatoms, pollen, and other organisms found in the sediments. It is proposed to analyze extensively a core extending 109 feet from the surface to underlying bed rock.

The project is designed as a collaboration among many fields: Sheldon Judson will work under the direction of Dr. Kirk Bryan; Drs. Elso Barghoorn, L. R. Wilson, Fred B. Phleger, David Linder, and others, will supply significant contributions; Mr. Frederick Johnson of the Peabody Foundation is acting in a general supervisory capacity.

Mr. Douglas S. Byers plans to conduct a limited reconnaissance and sampling of shell heaps in Hancock County, Maine, with a view to securing comparative data for the study of materials secured from the Nevin shell heap, in Blue Hill.

The Symposium Volume, tentatively titled "Man in Northeastern North America," which will appear as volume 3 of the Papers of the Robert S. Peabody Foundation for Archaeology, has been in the hands of the printer since late in January. It is as yet impossible to say when the volume will be ready for distribution.

A study of Grassy Island in the Taunton River, Massachusetts, on which there is an Indian site now below sea level, is scheduled to go to press about August 1. The paper is a collaboration between Frederick Johnson and Hugh M. Raup, with a study of the pollen by Edward Deevey. Analysis of the peat overlying the deposit containing artifacts makes it possible to suggest that the island was rendered uninhabitable about A.D. 1250. Arrowpoints and other stone tools appear to belong to the poorly defined grouping in the East which has been called the "Coastal aspect."

The second season of excavations on the Wickham site at Brewerton, New York, conducted by the Rochester Museum of Arts and Sciences, under the active direction of Dr. William A. Ritchie, was completed in September, 1945. Termination of work on this stratified station on the Oneida River brought to a close five seasons of explorations at the "prehistoric capital of New York." The amazing culture sequence revealed on seven sites at Brewerton is given in Dr. Ritchie's report on the Wickham excavations, now ready for the press. The relative chronological position of five major cultures is demonstrated by clear stratigraphic evidence.

On the Wickham site a component of the Canandaigua Focus (Owasco Aspect), rich in pottery and bone work, occurred in a deep brown sand layer overlying a black sand stratum bearing the component of the Point Peninsula Focus (Vine Valley Aspect). Over part of the occupied area the later residents had obliterated the older level intermixing its contents with their own distinctive goods.

In collaboration with the New York State Museum a part of the summer of 1946 will be spent by the museum party, under Dr. Ritchie, on Howland Island on the Seneca River, Cayuga County, in an effort to define more precisely the Point Peninsula Focus. Later several burial mounds probably of the Hopewellian Phase will be investigated in southwestern New York.

The Rochester Museum will also collaborate with the Indiana Historical Society which will sponsor a preliminary survey of southeastern New York with the purpose of determining, if possible, the archaeological culture of the Munsee group of the Lenapé Indians. Endeavors to locate documented sites as a point of departure will be made by Mr. Edmund Carpenter under the general supervision of Dr. Ritchie.

(*W. A. R.*)

GREAT PLAINS

Archaeological research in southern Manitoba was continued during 1945 by Chris Vickers. The work was stimulated by a generous graint-in-aid from the Historical and Scientific Society of Manitoba. Valuable assistance in the field was given by Nettie M. and Ivan Vickers; Dr. Douglas Leechman of the National Museum of Canada spent a week in the area during September. Dr. Leechman's visit was of inestimable value; without his aid and encouragement the ultimate worth of the years work would be measurably less.

A reconnaissance survey was made of the area south of the Assiniboine River between longitude 98° west and longitude 100° west. A preliminary archaeological map of the district was completed, with exact locations for all known sites fixed thereon. As a result of this survey, several hitherto unknown sites were located. Other sites, known to local collectors for the past sixty years, were visited and the local collections wherever available were examined. The majority of these sites did not yield any pottery, test excavation failed to reveal occupational deposits; they leave the impression of being the occasional camp sites of nomadic peoples, and in two instances white contact material indicated occupancy within the historic period.

Two exceptions to the above are worthy of mention. The Wightman Site near the Village of La Riviere (N.W. ¼ Sec. 7, T. 3 N., R. 9 W.) is rich in both pottery and chert artifacts. The site is on the edge of a deep ravine that is tributary to the Pembina River, and an unopened burial mound lies less than half a mile away. The pottery and artifacts in the Wightman Collection bear a marked resemblence to the material from the Avery Site mentioned later. The crest of Star Mound west of Snowflake (Sec. 22 & 27, T. 1 N., R. 10 W.) yields similar material, and there is some reason for assuming that both sites were villages of a people kindred to the occupants of the Avery Site. Star Mound is a low flat-topped elevation covering an area of about three hundred acres. There is a burial mound on the summit of the hill, marked today by a triangulation station of the Geodetic Survey of Canada. Dr. O. G. Libby has stated in the editorial introduction to "The Journal of La Verendrye, 1738–39" (*North Dakota Historical Quarterly*, Vol. 8, No. 4) that the south slope of Star Mound is the site of an earth lodge village of the Hidatsa. Trenching of three of the supposed hut-rings failed to confirm or refute Dr. Libby's assertion.

The greater part of our time was devoted to additional work on three sites already the subject of intensive study and mentioned in Vickers' paper in AMERICAN ANTIQUITY, Vol. XI, No. 2. A careful surface survey of the Kreiger Site (S.W. ¼ Sec. 27, T. 5 N., R. 16 W.) was completed in early June. This site was occupied within the historic period, and some progress has been made in establishing the tribal identity of its occupants. The Lowton Site (S.W. ¼ Sec. 26, T. 5 N., R. 16 W.) and its material received further study and comparison. This site has some connection with cultures of the upper Missouri River, but the details are still obscure. On the Avery Site (S.W. ¼ Sec. 14, T. 3 N., R. 13 W.) fourteen hundred square feet were excavated. As a result of this excavation, 1148 potsherds, 252 stone artifacts and 29 artifacts of bone and tooth were recovered. These, together with other cultural material, will provide a basis on which to build a comprehensive descriptive report.

(*Vickers*)

THE SOUTHWEST

The field work of the Arizona State Museum and the Department of Anthropology of the University of Arizona has included the following programs:

1. Excavations in the Bluff Ruin, an early Mogollon site in the Forestdale Valley were carried out during the summer of 1944. Tree ring dates place the time of occupation at approximately A.D. 300; the plane of culture was extremely primitive. Habitations consisted of circular, semi-subterranean structures entered through the side. These were excavated into the sandstone bedrock to a maximum depth of 5 feet. The material culture was sparse in all categories, including some pottery of plain brown ware but no types indigenous or intrusive which can be related in any way to the Basketmakers. The report on this work, coauthored by E. B. Sayles and E. W. Haury, is ready for publication.

2. During the summer of 1945, the Arizona State Museum carried on an archaeological survey in the eastern half of the San Carlow Indian Reservation with the idea of determining whether or not the resources of this area would warrant a long range research program. Visits were made to some 200 sites which represent various time periods between about the beginning of the Christian era and A.D. 1400. In addition to this apparent long prehistory, the survey also showed that the basic culture appears to be Mogollon with a strong late Anasazi overlay, and that throughout the whole range of occupation rather intimate contacts were maintained by these two groups with the Hohokam. On the strength of these preliminary findings an excavation program is proposed which will run for not less than ten years and will be operated as a field school in archaeology by the University of Arizona. Study will be inaugurated in June, 1946 with a limited personnel.

(*Haury*)

Dr. E. W. Haury has recently returned from a reconnaissance trip of north Mexico, made in company with Dr. C. O. Sauer, of the Department of Geography of the University of California.

The summer program of the University of Denver includes the following plans.

1. Dr. E. B. Renaud will conduct a class of students on a tour of parts of the Southwest, visiting the Cuchara sites near Walsenburg, Mesa Verde, Chaco Canyon and some of the eastern Pueblos.

2. Field work begun in 1942 and supplemented in 1944 and 1945 will be continued during 1946, in an effort to determine the types of culture and distribution of sites of upper Rio Grande areas of northern New Mexico and southern Colorado. Archaeological papers numbers three to six, of the University of Denver, have reported the earlier work. The summer's program will determine whether a supplementary report is essential.

(*Renaud*)

The Department of Anthropology of the Chicago Natural History Museum, directed by Dr. P. S. Martin and Dr. John Rinaldo, will continue excavations at the SU site and any other early or earlier site encountered during the summer of 1946. Reports of the work will, as usual, be published by the Museum.

(*Martin*)

The University of New Mexico Sixteenth Annual field sessions program is divided between two parts:

1. A group of beginning students will live on the campus at Albuquerque and will travel each day to two different sites, a pleistocene cave and a surface site on the Rio Puerco of the East. The cave is considered to be the primary excavation; reconnaissance investigation shows no Ansazi remains. Excavations at the surface site are undertaken in order to throw light on sequences of types of pottery in this region during Pueblo III and earlier times.

2. A small group of graduate and senior students will make a tour of the southern archaeological sites of the Southwest, and undertake limited excavation in caves close to the Mexican boundary, in southwestern New Mexico. Both projects will be directed by Paul Reiter.

There is little hope that the Southwest Museum will send an expedition into the field. Dr. M. R. Harrington is still recovering after having been struck by an automobile last November.

W. S. Stallings, Jr., Director of Research of the Taylor Museum at Colorado Springs, has been visiting institutions of the Southwest in order to select specimens for a Taylor Museum exhibit. He has now recovered from serious injuries sustained in an automobile smash-up near Tucson.

J. W. Hendron has completed a report dealing with the investigation of a small black-on-white site on the Pajarito Plateau west of Santa Fe.

(*P. W. R.*)

PACIFIC COAST AREA

Marian W. Smith reports that in the summer of 1945, Columbia University sent a Student Field Party, under her direction to the middle Fraser River, in southern British Columbia. The group was mainly interested in ethnology, but during the course of investigations several pit houses were located and mapped. Three days were also spent in excavation of a rectangular pit house on a site about three miles from Harrison Hot Springs. This is the first excavation of the kind to be carried on in the area.

Erna Gunther, Director of the Washington State Museum writes to say that the Department of Anthropology of the University of Washington will conduct a field course in archaeology in the San Juan Islands this summer. This will be under the direction of Arden R. King. It is hoped that this will be the beginning of a long term program of research which will include the entire North Pacific coast. Archaeology in this area has been a long felt need from the beginning of Boas' comprehensive studies.

Since the reconnaissance work of H. I. Smith and Gerard Fowke in the early 1900's and the survey of Philip Drucker in 1938 represent the only systematic archaeology done in the area, it is hoped that by beginning in the San Juan Islands some information will be gained concerning the culture history of the region comprising the Gulf of Georgia and Puget Sound. Especially important is the relationship of this area to the Fraser River valley. This has particular reference to the movements of cultures along this valley. Since these islands are near the mouth of the Fraser, this is a logical place to look for cultures which will indicate whether the direction of cultural movement was up or down the Fraser or both.

From L. S. Cressman comes word that a program involving a month's field work in the John Day region of north central Oregon will be undertaken by the Oregon State Museum of Anthropology of the University of Oregon in an effort to determine, if possible, the connections of the cave culture of the south central part of the State with the Columbia River region to the north. The distribution of L-shaped scapula awls and some basketry found in caves in north central Oregon indicates a connection with the Columbia River region, but the direction of flow represented by this distribution is not known. In addition to excavating at least one cave, the party will look for evidences of occupation in terraces of Pleistocene and Early Recent age along the John Day River.

The region of the John Day is critical for a test of the hypothesis of northern entry into the Great Basin from the region of the Columbia River. The Deschutes Valley might be a point of departure from the Columbia, but, if followed to its source, it would lead migrants into the Klamath Lake region and not into the Great Basin. The other possibility of entrance—by way of the Snake River—would bring migrants into the eastern part of the State and then into the Basin region to the south, if they did not follow the Snake to its confluence with the Columbia. On the other hand, they might follow passes leading from the Snake through southwestern Idaho into northeastern Nevada.

A grant-in-aid supporting this work has been made available by the American Philosophical Society to supplement a research grant from the University of Oregon.

A memorandum from Arthur Woodward concerning the state of archaeology in the Los Angeles Museum reports that all archaeological work of that Museum was curtailed during the war. The latest ventures were on Santa Rosa Island in November and December of 1941. The Museum intends to resume its survey of the islands off the coast of southern California—San Nicolas, Santa Rosa, San Miguel, and possibly Santa Cruz.

An archaeological project is also under way for northern Arizona during the summer of 1946. The Museum's last efforts in that region were 1930. Plans call for resumption of work on Pueblo III sites, probably in the region of Kayenta.

Malcolm F. Farmer, Acting Director and Curator of the San Diego Museum of Man reports that it may be the better part of a year before the museum buildings, which have been in use by the Navy since March, 1943, and not as yet rehabilitated and turned back to the city for museum use, can be reopened. Meanwhile the staff is concerned with plans for reopening. Mr. Farmer has been working on reinstallation plans for the whole museum, reorienting the museum program, developing an educational program, and caring for collections since his return from the Army on November 1, 1945.

Mr. Rogers resigned as Acting Director and Curator on December, 1945, and is now a Research Associate. He plans to live on his ranch at Jacumba and do some writing. He has made a few trips into western Arizona and southern Nevada, on one of which he found a new site on the shores of a dry lake in southern Nevada. It was rich in flaked stone artifacts of San Dieguito, Amargosa, Yuman, and other types.

The Santa Barbara Museum of Natural History has carried out further work on San Nicolas Island and progress has been made in the study of the material.

(*E. W. G.*)

MIDDLE AMERICA

The following account concerning new evidence of early man in Mexico was received during May, 1946, from Dr. Helmut de Terra:

"In the fall of last year the Viking Fund Inc., of New York, made it possible for me to go to Mexico and Guatemala. The object was to study the Late Quaternary Period during which early human occupation of North America took place. Since no cultures older than the Archaic or Middle Culture Period had been found so far in these countries my task was twofold: to find evidence of Stone Age Man and to work out a chronology for the Late Quaternary.

"Two cultures were found which antedate the known pottery horizons by long time periods. One is represented by a small number of scrapers, flakes and chipped pebbles of obsidian and chalcedony and a pointed bone tool. They were collected at three localities in alluvial and beach formations containing remains of mammoth, bison, horse and other land mammals of the Upper Pleistocene. The nature of the deposits indicates that climate was still wet and the lake at a high level. The second industry is represented by 65 implements of basalt of which a few were extracted from a fossil caliche soil and from ancient valley fills of Early Postglacial age. The soil represents a dry period following the last Pluvial. The implements consist of choppers, cores, high keeled and flat discoidal scrapers, end scrapers and hammerstones. The collection includes the fragment of a projectile point made of basalt.

"The geologic dating of these pre-pottery industries was

based on records left by fluctuating lake levels as well as on glacial chronology. The latter was worked out on the higher slopes of the volcanos Ixtaccihuatl and Popocatepetl. Two major glaciations were recorded which may be correlated with two Upper Pleistocene formations, Tacubaya and Becerra, in the Valley of Mexico. A Postglacial ice advance in turn corresponds with a high lake level in the Valley. On the basis of analogy with the Pluvials of the Great Basin the older industry would seem to date back to about 15,000 B.C. while the younger basalt industry cannot be much older than 10,000 and not younger than 5,000 B.C. The skull of Peñon, found in 1884 by Barcena and del Castillo, must, in the light of my studies, be considered a fossil man of either early Postglacial or even Upper Pleistocene age.

"No traces of early man were encountered in Guatemala though I did find a fossil land mammal fauna in the Motagua Valley which appears to be synchronous with the fauna with which early man in Mexico was associated."

On March 11, 1946, the United Fruit Company announced that it was undertaking a five-year project of archaeological research in the Guatemala Highlands, the principal objective being to excavate and restore some of the late and relatively well-preserved ruins in the region. In the early Spring of 1946 work was begun at the site of Zaculeu, three miles from Huehuetenango and 175 miles northwest of Guatemala City. The operations have been under the direction of John M. Dimick, with Stanley H. Boggs as assistant. Dr. A. V. Kidder is acting in an advisory capacity for the United Fruit Company.

A brief account received from Robert Smith states that during the first season's work at Zaculeu a fair amount of reconstruction was accomplished and one small tomb was discovered.

Robert Smith also reports that Ledyard Smith has finished his survey of the Quiché. In the process of mapping some mounds on the outskirts of Nebaj, Department of Quiché, he located a tomb which was nearly ten meters down from the surface of a low platform. It was difficult digging but well worth the effort, for the tomb contained both jades and fine pottery.

The Instituto de Antropología e Historia de Guatemala has recently been formed, set up in a manner similar to that of Mexico, and should do much to increase knowledge and interest in both the anthropology and Colonial history of Guatemala. The new Museo Nacional de Arqueología y Etnografía is nearing completion and its inauguration is planned for the near future.

(*G. F. E.*)

SOUTH AMERICA

(The following is a supplement to "Notes and News" published in a previous issue (Vol. XI, No. 2, October-December, 1945) on archaeological activities in South America since January 1943).

Peru.—Marian Tschopik writes that between January and September of 1943 Mr. John Rowe, then a member of the faculty of the University of Cuzco, took student field parties on several reconnaissance trips in the general region of Cuzco and also directed a two weeks' dig near Yucay in the Urubamba Valley. Late in 1943, Sr. Oscar Nuñez del Prado, a student of Rowe's, did reconnaissance near Curahuasi in the Department of Apurimac.

After Rowe's departure from Cuzco, Dr. Jorge Muelle took over teaching duties at the University of Cuzco, remaining there until the end of 1944. In February of 1944, Muelle made a brief survey trip south of Cuzco.

In the past two years Mrs. Tschopik has worked over collections from the excavations which she had previously made at Inca Uyu in Chucuito, and has, also, prepared a report on reconnaissance work in the Peruvian Highlands as a part of the program for 1941–42 of the Institute of Andean Research.

Mr. Harry Tschopik writes from Peru concerning an apparently early pre-ceramic site some 14 km. from Huancayo, near the Carnegie Institution Observatory of Terrestrial Magnetism. The site is remarkable for the apparent absence of ceramic material and abundance of stone implements and chips as well as bones and charcoal. This is unusual because sherds greatly outnumber stone and bone implements at such "early" sites as Chanapata, Pucara, Tiahuanaco, etc. Mere mention of such a site in the central Andean area presages a discovery of great importance.

Tschopik has prepared a short article on the rock shelter for publication in the October number of AMERICAN ANTIQUITY.

The Virú Valley Program of the Institute of Andean Research was initiated in the field in the first week of April of this year. It is planned to conduct a cooperative and integrated study of several aspects of prehistory that have, heretofore, been somewhat slighted in Peruvian archaeology. In conjunction with the usual chronological and distributional studies, research objectives are such important problems as changing patterns of land use during the prehistoric occupancy of the Virú Valley, settlement patterns, architecture, and public works such as roads and irrigation canals. Work of the past few months has already produced some interesting results and the future of the program is extremely promising.

Present participants include: Dr. W. D. Strong, of Columbia University, assisted by Mr. Clifford Evans; Dr. Wendell C. Bennett, of Yale; Dr. Gordon R. Willey, of the Smithsonian Institution; and Mr. James A. Ford, a Guggenheim Fellow. Mr. Junius Bird, of the American Museum, and Mr. Donald Collier, of the Chicago Museum, will join the party some time in the month of June.

The program, which was planned as the first intensive archaeological and geographical study of a single Peruvian coastal valley, is going forward along several different lines of approach. The Columbia University group, headed by Strong, is working primarily with the problem of vertical stratigraphy in an attempt to correct and amplify the archaeological chronology for the valley. Although results are as yet very tentative, Strong's party has lengthened the lower ranges of the sequences by going back to the Coastal Chavin horizon and, possibly, to an even earlier pre-pottery stage for the valley. Other new leads include a strongly implied new placement for the Virú Negative culture which now seems to take a position antecedent to rather than following the classic Mochica or Early Chimu style.

Willey and Ford have been pooling their efforts on a survey of the valley with Willey making a study of community patterns as they existed through prehistoric time and Ford conducting a pottery survey aimed at dating as many sites as possible in terms of the sequences being simultaneously worked out by the Columbia group. Preliminary results of this survey are nicely suggestive of a development of community aggregates, beginning with small villages of scattered single-unit houses and terminating with

great compounds of the Late Chimu period. Canals, roads, and other large scale public works have served to make this type of archaeological interpretation extremely fascinating.

Bennett, who has just arrived, plans a comparative study of architecture combined with an excavation program; Bird will concentrate his interest in the problem of the pre-ceramic horizon all along the Peruvian north coast; and Collier may enlarge the scope of the program by working both in the Virú and the adjoining Chao Valley.

Argentina and Brazil.—Dr. Wendell C. Bennett of Yale University has just completed a two months tour of Brazil and Argentina. During this period Dr. Bennett visited museums in both countries and consulted with anthropologists in Rio de Janeiro, São Paulo, and Buenos Aires. In the latter city he conducted a brief seminar discussion with Argentina's leading archaeologists.

Ecuador.—An interesting item which has not been reported upon has been sent in by Dr. Kalervo Oberg, now with the Institute of Social Anthropology of the Smithsonian Institution.

In the fall of 1942, Dr. Oberg, then a member of the Coordinator of Inter-American Affairs El Oro Rehabilitation Project on the Southern coast of Ecuador, had the occasion to come across some very interesting archaeological sites. This information will arrest the attention of Andean South Americanists as these sites are located in that scientifically unexplored area below the Gulf of Guayas and north of the Peruvian desert. In this region, south of the town of Puerto Bolivar and north of the village of Zarumilla, just above the Peruvian border, there are a number of large shell mounds situated in the coastal mangrove swamps. In terms of natural environment this is a crucial location as it is at just about this point that the coastal deserts give way to the tropical mangrove swamps.

The mounds were brought to Dr. Oberg's attention by local workmen who advised of their presence when questioned concerning sources of shell fill needed for construction work. Fortunately, adequate fill material was found nearer Puerto Bolivar, and the mounds were spared destruction.

Dr. Oberg describes one of the largest of the mounds as being an island in the swamp, about 5 acres across at the base and with the surface broken by numerous small hummocks and rises. In an old excavation he found a great many sherds, some of which bore painted designs. The presence of animal bones, ash, and other detritus indicates that the mounds were, at least in part, old habitation sites. From the photos which he has in his possession, it is clear that the mounds stand several meters above the surrounding tidal swamps. Their proper excavation would be a considerable job but one that should prove very rewarding.

Venezuela.—Dr. Walter Dupouy, Director of the Museo de Ciencias Naturales, submits the following concerning recent work at the sites of El Palito near Puerto Cabello. These two sites were first revealed by railroad construction operations in October 1944, and Srs. Dupouy and Cruxent proceeded to make preliminary investigations. On a later visit to the sites they were joined by Dr. Antonio Requena. Large scale excavations were begun in January 1945, after a number of test cuts had been made to determine the nature and extent of the site. Potsherds and artifacts of stone, bone, and shell were extremely abundant. In addition to the work at El Palito, a survey was made of neighboring sites, including the shell mounds at Sanchón and a station near Taborda. Material from the tests at the latter site was found to be quite distinct from that at El Palito.

The El Palito sites were both villages. The types of ceramics discovered there have not previously been found in north central Venezuela. The material has relationships to pottery from such sites as Barrancas, Estado Monagas, Ronquín, and Estado Guárico. There is some similarity to the more atypical specimens from La Cabrera in the region of Lake Valencia. There are also resemblances to the Santarem style of Brazil. A full length report on El Palito, by Requena, Cruxent, and Dupouy, is planned for the future.

Jamaica.—Mr. C. B. Lewis, Curator of the Science Museum, Institute of Jamaica, communicates the following with regard to his recent work in an aboriginal burial cave at Cambridge Hill, St. Thomas, and a village site between Spanish Town and Kingston. The cave was a large fissure in the limestone of the hill. The interior walls of the cave offered many natural shallow ledges on which burials were placed. The party recovered remains of at least 40 persons, many of which were accompanied with pottery bowls. One of the most interesting objects taken from this mass burial place is a wood carving of lignum vitae. The village site, on the Spanish Town-Kingston road, proved to have been, at least in part, occupied in post-Columbian times as bits of clay pipes and horse bones were found in the upper layers of the middens.

Mr. Lewis, in his letter, has generously invited any qualified parties from the United States to conduct archaeological investigations in Jamaica and to help in the field of Jamaican prehistory.

General.—At the Christmas meeting of the Institute of Andean Research, at Philadelphia, Dr. W. D. Strong was elected Chairman for the ensuing year. A Steering Committee composed of Drs. Strong, Bennett, Steward, and Willey was also selected for 1946. Dr. Gordon F. Ekholm, member and corresponding secretary of the Institute, was elected to the Board of Directors.

(*G.R.W.*

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